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RELATIVE TO THE

NATURAL HISTORY

O F

ANIMALS AND VEGETABLES.

Tranſlated from the Italian of the

ABBÉ SPALLANZANI,

Royal Profeſſor of Natural Hiſtory in the Univerſity of
PAVIA, Superintendant of the PUBLIC MUSEUM,
and FELLOW of various learned SOCIETIES.

To which are added

TWO LETTERS from Mr. BONNET to the AUTHOR.

And (to each Volume of this TRANSLATION) an APPENDIX,
the firſt containing a Paper written by Mr. HUNTER, F. R. S.
and the Experiments of Dr. STEVENS on Digeſtion; the ſecond
a Tranſlation of a Memoir of Mr. DEMOURS, and Mr. DEBRAW's
Paper on the Fecundation of Bees.

VOL. I.

LONDON:

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M DCC LXXXIV.

TO THE

BARON de SPARGES and FAIENTZ,

KNIGHT of the ORDER of ST. STEPHEN,

AND

COUNSELLOR to his IMPERIAL and ROYAL MAJESTY for the ITALIAN DEPARTMENT.

SIR,

YOUR Protection confers Honour on the Sciences, while your Cultivation extends them: You employ the Power entruſted to you by his Imperial Majeſty, in advancing the Happineſs of his People, by promoting the Progreſs of Letters, and diffuſing their kindly Influence. Your Attention unfolds the Abilities of Men, and creates a Multitude of uſeful Citizens. Your beneficent Activity affords a thouſand Reſources to all thoſe who devote their Talents to ſcientifical Purſuits; and with this View in particular, you enrich the Royal Cabinet of Natural Hiſtory at Pavia, by procuring from various Parts numberleſs Specimens equally rare and valuable.

Yes, Sir, it is You, in whom Europe beholds with Admiration the intereſting Spectacle of a Miniſter, who conceives that he is faithfully diſcharging his Duty to his Prince, while he enlightens at once the Country and Age in which he lives; and whoſe Relaxations from Toil, conſiſt in ſeeking in new Acquiſitions of Knowledge, the Means of increaſing the Number of happy Individuals.

Allow me therefore, Sir, to ſolicit your Indulgence for the Work which I preſume to offer, as a ſmall Token of my lively Gratitude for the many Favours which you have been inceſſantly heaping upon me. That Sentiment will be ever deeply engraven on my Heart, together with the profound Reſpect with which I have the Honour to be,

SIR,

Your moſt humble

and moſt obedient Servant

LAZARUS SPALLANZANI.

THE

TRANSLATOR'S PREFACE.

A LONG eſtabliſhed cuſtom ſeems to have conferred upon tranſlators, the right of annexing every epithet of praiſe, which language affords, to the name of their author. But of this privilege, however valuable, it is not my deſign now to take advantage. The celebrity of the favourite friend of a Charles Bonnet, and of the philoſopher, to whom the dying hand of Haller conſigned the defence of Truth and Nature, would not receive much addition from the encomiums of a writer, who chooſes to conceal his name. Yet as ſome uncommon experiments related in the following volumes, may draw the attention of perſons but ſlightly acquainted with Natural Hiſtory, I think it my duty to endeavour by ſome means to engage their confidence, leſt that which conſtitutes one of the chief excellencies of the work ſhould, by creating ſcepticiſm in the reader, prove a diſadvantage to it. Nothing appears more likely to ſhew what degree of confidence may be repoſed in the philoſopher, whom I introduce, than an enumeration of the ſervices which he has already rendered to this uſeful and amuſing branch of knowledge. And I undertake this taſk the more willingly, ſince I have obſerved that they are not very diſtinctly known, even to ſome who are employed in this country in purſuits of the ſame nature.

To ſever the different parts of animals, is one of the moſt effectual means of deſtroying them. If therefore I ſhould aſſert that this treatment will multiply ſome animals, and do no injury to others, the propoſition will probably appear, in the eyes of thoſe to whom this Preface is principally addreſſed, to have all the extravagance of fable. Nothing, however, is more certain. About the middle of the preſent century Reaumur, the great French philoſopher, diſcovered that ſome cruſtaceous animals have the power of reproducing the laſt joints of their legs. Soon afterwards, the regard of the world was more powerfully attracted towards this topic of Natural Hiſtory, by Trembley and Bonnet. From their obſervations it appears, that polypes, and ſeveral ſpecies of worms, are capable of forming anew thoſe parts, of which they have been deprived by deſign or accident. Some, however, of theſe animals are conſtructed with ſo much ſimplicity, and they all occupy ſo low a place in the ſcale of exiſtence, that Phyſiologiſts were not diſcouraged from attempting explications of phænomena ſo unexpected and ſo repugnant to received opinions. Their gelatinous conſiſtence, their ductility, their uniformity of organization, afforded a ſpecious foundation for the theory of animal reproductions (*a*). But the hiſtory of every ſcience ſhews, that at the dawn of new diſcoveries, the light is too feeble to guide Reaſon in her progreſs towards Truth. A ſhort time evinced, that other principles are neceſſary to the explanation of this wonderful property. In 1768, Spallanzani publiſhed his famous Proſpectus, in which he announced, that decapitated ſnails not only ſurvive this dangerous operation, but regenerate the part which they have loſt, and that the water-newt reproduces its legs and tail. He moreover aſſerts, that the ſame prerogative belongs to frogs and toads in the form of tadpoles. Ever ſince the date of the Proſpectus, the diſcovery concerning ſnails has

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(*a*) See Contemplation de la Nature, Partie 9.

been a ſubject of controverſy. The petulance of Voltaire, prompted him to level the ſhafts of ridicule againſt the Italian Naturaliſt. But he had more formidable opponents. The celebrated meteorologiſt, Abbé Cotte, Adanſon, the Hiſtorian of Senegal, and Bomare, the compiler of the Dictionary of Natural Hiſtory, concurred in attacking him. Their objections are founded upon numerous experiments, in all which the ſnails periſhed in very different times, indeed, after the ſeparation of the head, but without the leaſt token of reproduction. This contradiction impoſed upon the author the neceſſity of vindicating the reality of his diſcovery, and he accordingly, in the *Avant Coureur*, a French periodical paper, deſcribed the manner in which his experiments had been made. Nor does his teſtimony ſtand ſingle and unſupported: it has been confirmed by obſervers of no vulgar order; by Lavoiſier, by Shœffer, by Bonnet and Senebier, ſo that no weight can now be allowed to the negative reſults of thoſe who oppoſed the diſcovery. Though the work, which thus created a ſchiſm among Naturaliſts before its publication, has been ſo long delayed, it has not been abandoned. Its appearance may ſoon be expected. Beſides the curious experiments which I have mentioned,

tioned, it will contain many upon the earthworm. " Spallanzani, ſays Mr. Bonnet (*a*), has far outſtripped his predeceſſors. He firſt obſerved ſeveral ſucceſſive reproductions of the head in the ſame individual. He has ſeen pieces cut out of the middle of the body, become compleat worms, by producing a new head and a new tail. He has varied the tranſverſe ſections, and determined the points at which reproduction no longer takes place. He has aſcertained what length every piece muſt have, in order to regenerate a complete animal. He has traced with care the mode and progreſs of the regeneration. He has alſo varied the longitudinal ſections. He has attempted, but without ſucceſs, to form Hydras." It is therefore not unreaſonable to infer, that the novelty and variety of the experiments, will amply compenſate for the delay.

The next conſiderable work of Spallanzani, was a ſeries of obſervations on the circulation of the blood. On a function which has been ſtudied with ſo much diligence by ſucceeding phyſiologiſts, from Harvey to Haller, modern ingenuity could not be expected to throw much light. The illuſtrious anatomiſt, whom

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(*a*) Contemplation de la Nature, p. 249. Ed. nouvelle en 8vo.

whom I have laſt named, had excluded moſt of the auxiliary powers, which, according to preceding enquirers, concur with the heart in producing the perpetual motion of the blood. But the Italian Profeſſor, whoſe obſervations were made upon cold animals and on the chicken, attributes this effect to the heart alone. He allows nothing to the contraction of the veſſels or any other co-operating cauſe. He ſhews, that in arteries and veins of equal diameter, the blood moves with equal velocity. I need not inform the reader, that the rapidity of motion was very generally ſuppoſed to be much greater in the former than in the latter claſs of veſſels.

Many ſuppoſe, that the arterial blood moves more ſlowly, as it is at a greater diſtance from the heart. From his numerous obſervations, Spallanzani concludes, that it moves at leaſt very rapidly in the capillary veſſels of that ſpecies.

The velocity of the venous blood appeared to increaſe regularly from the largeſt to the ſmalleſt veins. In the latter the author concludes upon the whole, that it is three times as great as in the former. Though this deduction may ſeem in ſome meaſure to coincide with the doctrines of the mechanical ſect of phyſicians, yet the laws of hydraulics are very far from being ſtrictly obſerved here. For as the ſum of the diameters of the ſmall veins many times exceeds the ſum of the diameters of the large veins, the difference of velocity ought to be much greater.

The phænomena that offered themſelves to the obſerver's regard in the aorta, were new and curious. In the part contiguous to the heart, the motion of the blood was unequal, and even ceaſed during the time of diaſtole. This alternate ſtagnation became gradually leſs diſtinct, as the diſtance from the heart increaſed, and towards the more remote region of the aorta totally diſappeared. The velocity, however, was not ſo great towards the end of this principal artery, during the time of the dilatation of the heart, as during its contraction. This remarkable inequality vaniſhes altogether in the arteries of middle ſize, ſuch as the pulmonary and the meſenteric, and the motion of the vital fluid becomes quite regular and uniform.

Haller, whoſe obſervations on the chicken have ſo widely illuminated phyſiology, ſeems to have been deceived with reſpect to the primitive colour of the blood. The blood-veſſels have, according to him, a tinge of yellow. But the yolk, communicating its hue to them, deceives the eye of the obſerver.

ſerver. The addreſs of Spallanzani has diſſipated the illuſion. He detached the vaſcular membrane from the yolk, and ſetting it upon a plate of glaſs, found that the yellow colour diſappears, and is ſucceeded by a pure red.

Of the red particles of the blood he obſerved, that they do not turn round, according to a common ſuppoſition, upon their own axis, and that they have merely a progreſſive motion along with the other conſtituent parts of that fluid.

Thus the teſtimony of Spallanzani may be added to that of others, in order to prove, that the laws to which inanimate matter is ſubject, cannot be applied to living bodies. It could not, however, be expected, that mechanical phyſiologiſts would yield without a conteſt to this attack upon their theory. Accordingly Prochaſka, a phyſician of Vienna, has attempted to overturn theſe obſervations. But his objections will only ſerve to ſtrengthen the cauſe of his adverſary; and it would be eaſy, if this were the proper place, to expoſe their futility. It may perhaps be ſufficient to remark, that he has not taken up the microſcope, and ſeen the contrary of what is related by the Italian Profeſſor, but with a temerity that can ſcarcely be too much reprobated, has oppoſed reaſoning to experiment.

Thoſe diminutive beings that inhabit infuſions and the ſeminal fluid, have been more ſignalized than larger animals, by the diſputes and the theories of philoſophers. Since the æra of that diſcovery, which laid open a world before inviſible to human inſpection, Naturaliſts have enquired into their origin and nature with anxious curioſity, but with a degree of ſucceſs by no means proportional to their labour. The ſubject was involved in uncertainty and contradiction, when Spallanzani undertook to inveſtigate it; and in this enquiry, in which the union of genius and patience, invention and aſſiduity is ſo indiſpenſably neceſſary, it will not be difficult to ſhew, that he has ſucceeded better than any other obſerver. In the year 1765, he publiſhed *Microſcopical Obſervations relative to the Syſtems of Generation of Meſſrs. Needham and Buffon.* Continuing to ſtudy this curious ſubject with unremitting attention, he produced ten years afterwards a larger work, in which moſt of his former ideas appear confirmed by many repetitions, and amplified by important additions. Of this work I am about to gratify the reader, by a brief analyſis.

His firſt eſſays were made with a view to aſcertain the effects of heat. He boiled, roaſted, and even calcined ſeveral ſubſtances; but theſe violent proceſſes, when conducted in open veſſels, did not prevent the appearance of the animalcules. Nay, the infuſions of the ſubſtances that were boiled longeſt were the moſt populous for boiling, by haſtening their decompoſition, contributes to a more exceſſive multiplication. In veſſels hermetically ſealed, very different phænomena occur. The larger kinds (for theſe animalcules differ very widely in ſize) do not appear if the infuſion boils a ſingle ſecond; and ſo deleterious is the power of heat under theſe circumſtances, that they are not evolved in a warmer temperature than that which is marked by the 92 or 93° of Fahrenheit's thermometer. The more diminutive ſpecies ſuſtain, in cloſe veſſels, a boiling heat for half an hour, but not for three quarters. The author, deſirous of illuſtrating his ſubject by reſemblance and contraſt, was very naturally led to enquire how far other animals and vegetables are capable of reſiſting the effects of heat. He found in the firſt place, that the eggs of certain animals poſſeſs this privilege in a more eminent degree than the animals themſelves. Thus tadpoles and frogs periſh at 110°, 111°, whereas no degree of heat ſhort of 133 deſtroys their eggs (*a*). Silk worms, and the erucæ of the butterfly of the mountain aſh, are unable to ſuſtain the degree of heat expreſſed by 108°; but the eggs of both theſe ſpecies are not rendered unprolific by any degree lower than 133°, and many bear an hotter temperature with impunity. Large flies die at 99° $\frac{1}{2}$, their nymphs 110°, 111°, their maggots at 108°, and the eggs are ſpoiled only at 140°.

The ſame law extends to plants, and the ſeeds from which they ſpring. The ſeeds of the bean, the French bean and trefoil, ſuſtain unhurt the heat of boiling water, whereas the plants themſelves are deſtroyed by the temperature deſigned by the 167th° of Fahrenheit's thermometer.

The ſeeds of plants are more capable of reſiſting the action of fire than the eggs of animals. Of the former, all on which the experiments of Spallanzani were made, reſiſted a dry heat of 167°, and many of 212°. But not a ſingle egg was hatched after it had ſuſtained the 145th° of Fahreneit's thermometer.

(*a*) It will be ſeen in the ſecond Volume, that theſe bodies are not eggs; but this circumſtance does not in any way affect the reſult of the experiment.

I ſay a *dry heat*, becauſe it appears that fire, in combination with water, acts with greater efficacy.

Thus, if we except the larger animalcules of infuſions, the analogy between the inhabitants of the microſcopical and the viſible world is complete, and thus every new obſervation in Natural Hiſtory, affords a freſh example of the beneficent care of Providence. By what means HE, whoſe wiſdom has ordained, that the generations of created beings ſhould follow each other in a regular ſucceſſion, has ſecured this privilege to the rudiment of the future animal, who does not wiſh to be informed?—But its extreme minuteneſs keeps it ſo remote from every ſenſe of man, that the imagination is left to form conjectures unaſſiſted and alone. The difference, which is great, and conſiſts of many degrees of heat, forbids us to ſuppoſe that the covers, under which the germ is lodged, can be the cauſe of ſo remarkable an effect. Nor can its ſmallneſs be admitted as a ſatisfactory reaſon, ſince we cannot but ſuppoſe the particles of heat to be ſtill ſmaller. Is the phænomenon owing to the dimneſs of the ſpark of life? Analogy teaches, that this ſpark is leſs eaſily extinguiſhed in thoſe caſes where it is leſs vivid. Thus animals in a ſtate of torpor do not ſo ſoon die after violent operations, ſuch as decapitation, as when the vital energy is ſtrong; and inſects plunged in water, do not drown in winter ſo ſoon as in ſummer. The hypotheſis is not without plauſibility, and the Phyſiologiſt will ſuppoſe, without much reluctance, that the principle of life is leſs intenſe before fecundation.

The Author next paſſes, by an eaſy tranſition, to an examination of the effects produced by cold. He found that the germs of theſe minute beings reſiſt a very intenſe cold. The animalcules themſelves of a ſmaller ſpecies are not deſtroyed, as long as the water retains its fluidity. And even in this caſe, it is the congelation of the water, and not the abſence of heat which proves fatal; for in water, in which the thermometer ſunk 16° below the point of congelation, but which, by being kept at reſt, was prevented from being frozen, they ſurvived, though their movements became indeed leſs briſk. It deſerves to be remarked, that the ſpecies which ſuſtained this rigorous temperature, agreed perfectly in appearance with thoſe which bore the greateſt heat.

In the proſecution of his experiments, the author found, that the animalcules of infuſions are deſtroyed by the odour of camphor and turpentine, by the ſmoke of tobacco and the vapour of ſulphur, by urine and other ſaline ſubſtances. The electrical fluid alſo kills them inſtantaneouſly, and they agree with other animals in requiring the influence of the air for the ſupport of life. Some ſpecies, however, are entitled to the notice of the Naturaliſt, on account of the length of time during which they live in void ſpace. If ſome periſh in a day or two, there are not wanting others which can ſupport the abſence of air for a month. Nor do they immediately diſcontinue the exerciſe of their functions, or abate of the quickneſs of their motions.

Since experiment has ſucceeded to conjecture, and obſervation grown more accurate, men have diligently endeavoured to trace, by deductions from facts, the origin of animated beings. Among the ſyſtems which have been invented, ſeveral have been built upon the inhabitants of infuſions, and the ſeminal fluid. Writers, finding nothing in the production of large animals to favour their paradoxical opinions, have ſought for refuge in the obſcurity of minuteneſs. Needham aſſerts, that certain filaments, obſervable in infuſions, are animated by an internal expanſive five ſpirit, and that after ſwelling and performing certain irregular progreſſive motions, they divide into ſmall pieces, which being more and more exalted by the expanſive force that is continually purifying them, are at laſt converted into microſcopical animalcules. This wonderful tranſmutation of vegetables into animals, Spallanzani has diligently, but in vain, ſought. He has ſeen animalcules appear before filaments, he has ſeen animalcules in infuſions which never produced any filaments, or in other words, any mould, and he has ſeen filaments where there were no animalcules. Beſides theſe deciſive obſervations, he has diſcovered the ſource of Needham's errors. Among the animalcules of infuſions, there is a ſpecies diſtinguiſhed by a long filament at the poſterior part of the body, by means of which it fixes itſelf to the ſmall vegetable productions; this ſpecies is multiplied by the diviſion of one individual into ſeveral others. It muſt likewiſe be remarked, that among microſcopical vegetables, there are funguſes in miniature. They conſiſt of a large head ſupported by a ſlender ſtalk. The miſtake, therefore, which gave riſe to all theſe wild ideas concerning generation, was ſufficiently natural. It conſiſted only in confounding two productions, which, though they are ſo eſſentially different, do not ill agree in outward appearance.

The various ways in which the ſeveral ſpecies of animalcules are multiplied, are among the moſt curious phænomena which the microſcope diſcovers. Some divide into equal parts; the reader will have a clear conception of this mode of diviſion, if he will form to himſelf the idea of an inflated bladder, with a ligature thrown round the middle of it. This will repreſent the firſt appearance of diviſion. The tranſverſe furrow gradually becomes deeper and deeper, and at laſt the two individuals are connected only by a point. Then an unexpected ſcene paſſes before the eyes of the ſpectator. The other animalcules, though they at all times beſides carefully avoid impinging againſt each other, now ruſh, by one conſent, upon the place of diviſion, and the aſſiſtance they thus afford, together with the efforts of the united individuals, at laſt effects a ſeparation. This appearance of ſocial inſtinct, in beings placed at ſuch a diſtance from thoſe which we are moſt accuſtomed to contemplate, will perhaps ſeem apocryphal to thoſe to whom I am writing, ſo much has it the air of a tale borrowed from a deſcription of fairy-land. It is therefore proper to inform them, that it ſtands amply confirmed by the teſtimony of other naturaliſts of veracity equally unſuſpected. Nor are examples wanting of a natural diviſion among animals more eaſily acceſſible to the ſenſes.

Some ſpecies are multiplied by a longitudinal diviſion, which ſometimes begins at the fore part of the body, and ſometimes at the hind part. Thus far the diviſion is equal; but there are alſo examples of inequality, a ſmall piece only being detached from ſome kinds.

A ſuperficial ſtricture between two globules, may in ſome inſtances be perceived. The obſerver ſuppoſes that this ſtricture will, as before, become gradually deeper, till it terminates in a point, but is ſurprized by an inſtantantaneous ſeparation.

Groups conſiſting of four or five globules, are often ſeen to float for ſome time in an infuſion, and afterwards to ſeparate into bodies, which are at firſt round, but in time are ſplit into lobes, that ſeparate in the ſame manner.

There is another mode of diviſion yet more extraordinary. Let the reader imagine a ſpherical body formed of concentrical ſtrata, of which each conſiſts of a number of animalcules. Thoſe which compoſe the ſuperficial ſtratum, part from the others, and ſwim about the liquor. The next in ſucceſſion do the ſame, and ſo on till the ſphere is decompoſed.

Theſe are not, as may very naturally be imagined, a cluſter of animalcules that unite and ſeparate at will. The author's obſervations ſufficiently prove the contrary, for when he kept one apart from the reſt, he found that it grew in three or four days to a perfect reſemblance of the ſphere from which it ſprang.

Of theſe modes of multiplication, it may in general be obſerved, that they are forwarded by heat, and retarded by cold. The diviſion which in winter requires ſeveral hours, is in ſummer effected in a quarter of an hour.

The inhabitants of infuſions afford oviparous as well as viviparous individuals. In all multiplication is exceedingly rapid. A ſingle individual ſoon produces an offspring too numerous to be counted; and the quantity in a ſhort time ſurpaſſes not only belief but imagination. "Their number, ſays the author, exceeds that of the integrant parts of the fluid. All putrefying ſubſtances, and water of every kind, contain prodigious multitudes. Every drop is peopled with myriads.

Let the reader now reflect upon the quantity of running, ſtagnating, and ſalt water, and then let him try to conceive the number of theſe animals."

If unſatisfied curioſity ſhould ſeek to penetrate yet a ſtep further into the origin of animalcules, the experiments of Spallanzani will ſupply the means of gratification. As the phials in which infuſions are contained are more or leſs cloſely confined, a ſmaller or larger number of animalcules appears in them. Hence it is probable, that the atmoſphere is the mighty magazine in which Nature has repoſited her immenſe ſtores of the germs of theſe diminutive beings.

The manner and variety of their movements deſerve likewiſe to be noticed. Some, like eels, proceed by the contortion of their bodies; points and filaments growing out of the body are the means by which others are enabled to advance. Some move ſlowly, others with a ſwift pace. Several ſpecies are diſtinguiſhed from all other productions of creative wiſdom, by being unacquainted with repoſe. In one ſpecies certain filaments placed at the poſterior part of the body, by being alternately bent, and ſuddenly made ſtreight again, project the animal to a great diſtance, as an arrow is ſhot from a bow. Some continually revolve like tops upon their own axis, but this rotatory motion is attended with a progreſſive one.

The means by which they ſuſtain life, are not the leaſt engaging part of their hiſtory. Some excite an eddy by moving rapidly the filaments that are placed round the opening of the mouth, and thus draw towards them the particles of food that float in the liquor. Others, with all the ferocioufneſs of rapacious animals, purſue their weaker fellows. In theſe, which may be termed animalcules of prey, it is pleaſant to behold the repoſe that follows ſatiety, and the exertions that are excited by hunger.

This ſketch ſhews, with ſufficient evidence, the falſehood of the principles upon which Needham has conſtructed his ſyſtem of generation. The animalcules of infuſions neither ſpring from vegetables, nor can they be conſidered as other than real animals.

Having atchieved the overthrow of Needham, Spallanzani proceeds to encounter a more formidable antagoniſt. Buffon, whoſe ſkill in compoſition has reſcued the French language from the imputation of feebleneſs and want of nerves, has founded a theory of generation on the corpuſcles, which the microſcope diſcovers in the ſeminal fluid. This fluid he affirms to be at firſt full of ramified filaments, which, after a certain interval, burſt and emit a number of ſmall oval bodies, attached to the filaments by a long thread. After ſome oſcillations they liberate themſelves and move about, though but ſlowly, on account of the incumbrance of the long thread or tail which they are obliged to drag after them. They afterwards loſe this thread, and then their motions become quicker. Some ſeem to change their ſhape and ſize, and then alſo the velocity of their movements grows gradually greater. The obſerver frequently ſees the oval changed into the globular figure, and one corpuſcle divided into two. Such are the obſervations from which the French naturaliſt concludes that theſe corpuſcles are not real animals. The loſs of their tail, the change of their figure, their formation under the eye of the obſerver, their diviſion and gradual diminution of bulk, are circumſtances which, in his opinion, concur to exclude them from the animal kingdom. But as they have an innate power of motion, and ſhew ſigns of vitality, he on the other hand ſeparates them from inanimate matter, and creates a particular order of exiſtences, under the title of *organic molecules*. To theſe molecules caſt in certain internal moulds, he attributes the formation of all animated beings.

But this theory can no longer ſubſiſt without other proofs, for Spallanzani has taken from it all the ſupport it received from the inhabitants of the ſeminal liquor. His obſervations on this ſubject, bear ſo cloſe a reſemblance with thoſe on the animalcules of infuſions, that I ſhall forbear entering into a particular account of them. He has brought irrefragable proofs that they do not ſpring from filaments, that they never loſe their tails, and that their ſize does not diminiſh or their activity increaſe in a regular progreſſion. He has ſhewn, by comparing their properties with the properties of other animals, that they have an indiſputable right to be ranked among the productions of the animal kingdom. Buffon deſcribes them as ſurviving ſeveral days after they have been expoſed to the influence of the air, but Spallanzani always found that they died in a few hours.

Concerning narrations thus contradictory, the determination of naturaliſts would be different, and each would become a partizan of the French or Italian philoſopher, according to his opinion of the veracity and ſkill in obſervation of the one or the other, if the latter had not diſſipated every doubt, by expoſing the origin of the other's miſtakes.

When the ſeed has been taken from its receptacles, and the ſpermatic worms have lived the ſhort ſpace afterwards allotted to them, this fluid, like every other animal ſubſtance, acquires new properties, and loſes thoſe which it before had. A new race of inhabitants ſucceeds ſooner or later with a ſhape, ſtructure, and movements, totally different from thoſe of the firſt, but agreeing with the form, which, according to Buffon, the organic moleclues aſſume. Agreeably too with his deſcription, the ſeveral ſucceſſions become gradually ſmaller, till at laſt the eye of the obſerver loſes them in indiſcernable littleneſs. Theſe occupiers of corrupted ſemen, are in no reſpect different from the animalcules of infuſions, but from the ſpermatic worms they are ſeparated by every mark which can enter into the deſcription of the naturaliſt. The ſtructure, the ſhape, the manner and velocity of motion, the place of reſidence, the effects of air, afford ſo many diſcriminative ſigns.

Before theſe experiments, the theory of Buffon vaniſhes as an enchanted caſtle at the approach of the deſtined knight. But it is pleaſing to obſerve, that while they ſhew the falſehood of his ſyſtem, and the inaccuracy of his obſervations, they are favourable to his veracity. In his deſcriptions truth is ſo entangled with error, as to ſhew that his judgment, and not his intention, was in fault. A ſuſpicion, not perhaps deſtitute of plauſibility, has been ſuggeſted, that he had ſeen the organic molecules with no other eyes than thoſe of the imagination. I therefore experience no common ſatisfaction, while I bear this teſtimony to the character of that ſublime and original genius, who, by painting the works of God in colours worthy their variety, their beauty and their grandeur, has rendered the Hiſtory of Nature more alluring than the fictions of poetry.

If I may be allowed to introduce an incidental remark, I would turn the attention of the reader to the difficulty, under which the moſt ardent imagination muſt labour, when it attempts to conceive the production of organized bodies, without the aid of ſome pre-exiſting cauſe, not eſſentially unlike germs. In whatever proportion the ſeveral forms of matter are combined, and in whatever manner the powers which Chemiſtry chiefly diſcovers that they poſſeſs, are ſuppoſed to exert themſelves, yet when a mind, neither influenced by the love of paradoxical opinions, nor actuated by the deſire of contradiction, tries to imagine how an animal can be formed from ſuch materials arranged by ſuch cauſes, it turns aſide from the ſpeculation in hopeleſs deſpondency. When Buffon ſubſtitutes his organic molecules and internal moulds, he unluckily propoſes a theory encumbered with all the difficulties attending the hypotheſis of germs, but deſtitute of its advantages.

In Spallanzani's experiments on the effects of confined air, his uſual accuracy may be recognized; but that originality of conception, which diſtinguiſhes his other works, is not ſo ſtriking in this eſſay. The public attention, at leaſt, has been ſo much turned towards elaſtic fluids, ſo much has been written concerning the effects of vitiated common air, and the ſeveral ſpecies of gas upon animal and vegetable life, that an analyſis of theſe experiments would be a repetition of what is to be found in a vaſt variety of publications. I will only obſerve, that he attributes the death of animals, kept in unchanged air, to exhalations acting on the nervous ſyſtem. If he is acquainted with the writings of Prieſtly, he will probably have found reaſon to give his hypotheſis a modification ſomewhat different.

Certain animals, which loſe every property and appearance that diſtinguiſh them from inorganic matter, and recover the exerciſe of their functions, have drawn the regard of Spallanzani. To the information afforded by Leuwenhoeck, concerning the rotifer, he has added many curious particulars. He has diſcovered ſome new ſpecies endowed with this remarkable property. He has ſhewn, that though they may be reſuſcitated after a long ceſſation of the powers of life, and a certain number of times, yet theſe qualities have their limits; nor does immortality belong to theſe extraordinary productions of Nature, any more than to other created beings. He has deſcribed the phænomena, the loſs and recovery of vitality, with minuteneſs and preciſion.

The chief aim of the experiments on moulds, is to trace the origin of that minute vegetable. It had been rendered probable by Micheli, the famous botaniſt of Florence, that mould is propagated by granules, ſhed in great abundance from the burſting heads of the plant. Againſt this opinion, Moſcati advanced ſome very ſpecious objections; and upon the whole concluded, that mould is the produce of ſpontaneous generation. But Spallanzani has determined the controverſy in favour of the Florentine naturaliſt, by many well-imagined and well-executed experiments; ſo that both microſcopical animals and vegetables agree in their origin with thoſe that have been conſtructed upon a larger model.

In the works which have ſupplied me with the foregoing obſervations, there are ſeveral incidental diſquiſitions, which will amply reward the attention of the reader. But I muſt omit them, leſt this preface ſhould be extended beyond all bounds. There is, however, one digreſſion ſo intereſting in itſelf, and at the ſame time ſo characteriſtic of the genius of the author, that I am tempted to produce it here.

Among the phænomena recorded in Natural Hiſtory, I recollect none more extraordinary, than that ſtate of torpor into which many animals fall at the ſetting in of the cold ſeaſon. By what means this great change is produced, whether the fluids are rendered unfit for circulation, whether the nerves are deprived of their influence, or whether the muſcular fibre is no longer ſuſceptible of that influence, philoſophers have not, till very lately, thought of enquiring. The great claſſical writer of France, the illuſtrious Buffon, firſt inveſtigated this curious topic of comparative phyſiology. He conſiders the temporary ceſſation of the powers of life, as ariſing from the refrigeration of the blood. From ſeveral thermometrical obſervations he concludes, that the temperature of theſe animals is equal to that of the atmoſphere, and regulated by it. But the Italian Profeſſor, having repeated theſe obſervations with his habitual accuracy, found, that the ſubjects of Buffon's experiments are not to be numbered among animals of cold blood. In their mouths he has ſeen the liquor of the thermometer riſe to the 101ſt degree, at a time when in the open air it ſtood at 35° or 36°. But the genius of Spallanzani did not reſt here. By an experiment which alone would have raiſed its inventor to an high rank among Naturaliſts, he at once compleated the overthrow of Buffon's theory, and aſcertained, at leaſt with great appearance of probability, the cauſe of the phænomenon. The tenacity of life by which frogs, toads, and other amphibious animals are diſtinguiſhed, is ſufficiently known. The heart of theſe animals may be cut out, not only without immediately deſtroying them, but without retarding the exerciſe of their functions, or impairing the vivacity of their movements. Taking advantage of this ſingular property, the author evacuated the veſſels of ſeveral frogs, newts, and toads. He then buried them in ſnow; the expected effect followed, and they ſoon became torpid. Being expoſed in this ſtate to a proper temperature, the ſuſpenſion of life was ſucceeded by a complete recovery of the uſe of their vital organs: inſomuch that there did not appear to be any difference between thoſe which had ſuſtained the loſs of their fluids, and thoſe which were ſubjected to the experiment unhurt and entire. The cauſe, therefore, of this lethargy, is not connected with any condition of the blood; it ſeems to depend on the privation of the irritability of the muſcular fibre: Spallanzani at leaſt obſerved, that no ſtimulants, not even the electrical fluid itſelf, produced the ſmalleſt contraction of the muſcles.

I am aware, that this enumeration of the productions of Spallanzani is imperfect; I have purpoſely omitted a tranſlation of the *Contemplation de la Nature*, and ſome ſmaller pieces, ſuch as ſeveral articles in the Proſpectus of the Siennese Encyclopædia. Notwithſtanding the freedom of modern intercourſe between different nations, and the advantages of commerce, foreign books are not ſo regularly imported into this kingdom, but that ſome original and important work may

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have eſcaped my enquiries. But the rude ſketch which I have exhibited, will, I hope, fulfil my wiſhes, and induce thoſe who have no better means of information concerning the worth of this diſtinguiſhed philoſopher, to hear him with reſpect and attention.

Of the following work, it is not neceſſary to attempt an analyſis. Its importance cannot fail to ſtrike the moſt careleſs obſerver. In the firſt volume, we have a complete hiſtory of one of the chief functions of the body. The ſecond diſſipates much of the darkneſs that has brooded for ſo many ages over the proceſs of generation. When, however, I view theſe ſplendid diſcoveries in their brighteſt light, when I conſider them in their relation to the art, of which the object is the health of mankind, I cannot but own with regret, that they have rather a negative than a poſitive merit. Like other great advances in phyſiology, they ſerve rather to extirpate error, than to afford materials of very high value for the true theory of medicine.

In executing the following tranſlation, my principal aim was to give the preciſe meaning of the author. My ſecondary object was, to preſerve unſullied the purity of my native language.

The firſt of theſe intentions, I hope, I have fulfilled; and I may, I think, venture to aſſure the reader, that he may conſider the following work as the genuine repreſentation of the original. It may, however, be neceſſary to apprize thoſe, who ſhall compare the Engliſh with the Italian, that I was compelled, however reluctantly, to take ſome liberties with the author's phraſeology. His deſire of being perſpicuous, has ſometimes led him into diffuſeneſs. There occur too frequently expreſſions which clog the ſentence, without introducing any new idea, or illuſtrating any propoſition already advanced.

This defect did not fail to ſtrike me on the firſt peruſal of the Diſſertations. But, perſuaded that the tranſlators of books of ſcience and of polite literature, ought to hold very different objects in view, that exactneſs is the buſineſs of the one and elegance of the other, I tranſlated the firſt diſſertation very cloſely. But when I came to conſider my attempt, I immediately perceived, that the ear of no reader would tolerate it; and ſome judicious friends concurring with me in opinion, I threw it aſide, and began the work again, with a determination to prune this uſeleſs luxuriance. I had performed more than half my taſk, before I met with a paſſage in a judicious author,

thor, which at once confirms my ideas, and juſtifies the freedoms I have taken. Mr. Bonnet, in a letter to Spallanzani himſelf, uſes the following terms. "Vous êtes, en general, très-clair & très-méthodique. Je vous exhorterai ſeulement à vous reſſerrer un peu plus dans certaines deſcriptions, où vous employez plus de mots, qu'il n'en eſt beſoin. Evitez encore les pléonaſmes & les ſynonimes: ils n'ajoutent rien à l'idée (a).

That I have attained my other purpoſe I dare not affirm. When the mind is moulded to the idiom of one language, it cannot immediately receive a new impreſſion without retaining ſome traces of the former. On this account, it may ſafely be aſſerted, that tranſlation, as far as mere expreſſion is concerned, is the moſt difficult ſpecies of compoſition. When I read what has been printed for ſome time, I eaſily perceive, that many phraſes might be made neater and ſmoother, though not perhaps more perſpicuous, which I conſider as the chief excellence of a work of this kind.

That I might the more effectually recommend my tranſlation, I have added ſome things, which are not to be found in the original.

(a) Œuvres ed. in 8vo. tom. XI. p. 322.

The experiments of Dr. Stevens will doubtleſs be well received. Such academical productions are ſeldom very widely diffuſed, and I have heard many wiſh for a peruſal of them. I have not tranſlated the whole diſſertation, for the former part did not ſtrike me as new, or peculiarly excellent. I will not, by pointing out the circumſtances, in which they agree or diſagree with thoſe of Spallanzani, deprive the reader of an amuſing employment.

If Mr. Hunter's eſſay had been more remotely connected with the ſubject of the firſt Volume, I would have cauſed it to be reprinted. It contains ſo many uſeful views, it is ſo pregnant with the ſeeds of future diſcoveries in phyſiology, that the attention cannot be too often directed to it.

The papers of Mr. Demours and Mr. Debraw will, I hope, be read with pleaſure at the end of the ſecond Volume. They are in themſelves curious, and bear a near relation to the contents.

It was my deſign to have annexed a tranſlation of Reaumur's Memoirs on Digeſtion; but I found, that the very ample account given of them by the author, was quite ſufficient for the information of the reader.

That nothing might be wanting, which it was in my power to ſupply, I have tranſlated the ſeveral Latin, French, and Italian paſſages, which it ſeemed proper to retain in the text.

INTRODUCTION.

IN the courſe of my public demonſtrations in the year 1777, I repeated in the preſence of my hearers thoſe celebrated experiments of the Academy of Cimento, that ſhew the aſtoniſhing force with which the ſtomachs of fowls and ducks pulverize empty globules of glaſs in the ſpace of a few hours. Finding them perfectly exact, I conceived the deſign of extending them to ſome other individuals of that claſs of birds which have been termed birds with *muſcular ſtomachs* or *gizzards.* Such were the firſt lines of an undertaking, of which till that time I had never entertained the ſmalleſt idea, and which afterwards increaſed in proportion to the increaſe of my curioſity concerning a ſubject of ſo much beauty and utility as the important function of Digeſtion. Hence from animals with muſcular, I was induced to proceed to thoſe with intermediate, and from theſe to animals with membranous ſtomachs (*a*). Thus I enjoyed the pleaſure of extending my reſearches to the principal claſſes of animals, not neglecting Man, the nobleſt and moſt intereſting of all. But theſe phyſiological reſearches laid me under the neceſſity of examining the moſt celebrated ſyſtems concerning Digeſtion, and of enquiring whether it is effected by trituration, by a ſolvent, by fermentation, or by an incipient putrefaction: or whether, according to the opinion of the great Boerhaave, it rather depends upon all theſe cauſes operating in conjunction. Thus I was obliged to enter anew upon a queſtion of very ancient date, and though diſcuſſed at great length by many phyſiologiſts, yet not in my opinion ſufficiently elucidated; ſince moſt writers have choſen to follow the deluſive invitation of theory and hypotheſis, rather than the unerring direction of deciſive experiments. The impartial and judicious reader, when he ſhall have peruſed the preſent eſſay, will be able to determine, whether what I aſſert, is true or falſe.

(*a*) The I, LVIII, and CIV paragraphs will explain what is meant by birds with muſcular, intermediate, and membranous ſtomachs.

DISSERTATION I.

CONCERNING

DIGESTION.

ON THE DIGESTION OF ANIMALS WITH MUSCULAR STOMACHS, COMMON FOWLS, TURKEYS, DUCKS, GEESE, DOVES, PIGEONS.

I. THOUGH there perhaps exiſts no animal, of which the ſtomach is not furniſhed with muſcles, yet there is a ſingular claſs, juſtly denominated by ſeveral naturaliſts *animals with muſcular ſtomachs*, ſince that viſcus is provided in a remarkable manner with very large and powerful muſcles. To this claſs belong fowls, ducks, pigeons, geeſe, partridges, &c. So great is the ſtrength of theſe muſcles, that many have imagined that they produce digeſtion by acting violently upon the contents of the ſtomach, and breaking down and reducing them to a pultaceous maſs, in no reſpect differing from imperfect chyle. This notion was afterwards applied to other animals, nor was man himſelf exempted; and it has been pretended, that digeſtion is univerſally owing to the alternate action of the muſcles of the ſtomach, or, as it has been termed, to *trituration*.

II. Now, to confine ourſelves to animals with muſcular ſtomachs, there was little difficulty in deviſing the means of determining whether the comminution and ſolution of food is effected by the gaſtric muſcles. Such means have been invented and ſucceſsfully applied by Reaumur. Let ſeveral animals reſembling each other in ſtructure, ſays that great naturaliſt in his two excellent memoirs on this ſubject, of which I ſhall make frequent uſe in the ſequel, be obliged to ſwallow metallic tubes open at both ends, and filled with ſome of their natural food, as grains of the *Cerealia* when gallinaceous fowls are the ſubjects of our experiments. Should theſe grains, after they have remained a certain time in the ſtomach, be broken down and decompoſed, we muſt aſſign a diſſolving liquor as the cauſe of the phænomenon, ſince the ſides of the metallic tubes muſt have been an inſuperable obſtacle to the exertions of the gaſtric muſcles upon the contents; but if they ſhould be retrieved in a ſound and entire ſtate, it muſt be acknowledged, that in theſe animals digeſtion does not depend on a ſolvent, but on muſcular action. Such were the means employed by this ſagacious naturaliſt. In ſome metallic tubes perforated at each end he encloſed grains of barley, and obliged common fowls, turkies, and ducks, to ſwallow them. Several hours afterwards the animals were killed, and the tubes taken out of the ſtomach, but the barley was found quite entire; whence he inferred, that in birds of the gallinaceous claſs the food is not broken down by a ſolvent, but by ſtrong muſcular action.

III. This experiment is highly favourable to the doctrine of trituration; yet I think it would have been much more concluſive, if the ſame reſult had been obtained from other individuals of this claſs, and if beſides barley, other grains upon which they naturally feed, ſuch as wheat, maize, rye, chick-peaſe, &c. had been employed. I therefore reſolved to put each of theſe ſeeds to the teſt of experiment in the following manner. I procured ſome tin tubes eight lines in length and four in diameter, and incloſed in each a number of the ſeeds juſt mentioned inverſely proportional to their ſize. The ends of the tube were left open, but iron wires were made to paſs before them, ſo as to croſs each other, and form a kind of lattice-work. Common fowls were the firſt ſubjects of my experiments: I forced ſome of the tubes into the ſtomach, conducting them with my fore-finger and thumb through the œſophagus, till I was certain they were in the cavity of that viſcus. When this operation is properly performed, neither fowls nor other animals ſuſtain any injury. In twenty-four hours the tubes were taken out, and the contents upon examination appeared to be unaltered: even the colour and taſte were unchanged, if we except a ſlight bitter flavour which they had acquired. They had imbibed a fluid, and were a little ſwoln. And the ſame ſeeds incloſed in tubes, and left in the ſtomach two and even three days, underwent no greater change.

IV. Sometimes, after having forced the tubes full of grains into the ſtomach, I immediately introduced ſome of the ſame grains looſe. The latter were broken down in a few hours, but the former remained entire.

V. The food taken ſpontaneouſly by theſe birds does not paſs immediately into the ſtomach, but ſtops for ſome time in the crop, where it is macerated, and becomes ſofter. Is ſuch a previous maceration neceſſary before it can be diſſolved within the tubes? This circumſtance ſeemed to deſerve attention. I therefore repeated the foregoing experiments with ſeeds taken from the crop of a fowl, after they had undergone a complete maceration. Notwithſtanding this preparation, they underwent no change within the tubes.

VI. From theſe reſults it was eaſy to predict, that no new appearance would occur, if the ſkin ſhould be taken off, as it really happened. It is proper to add, that other grains treated in the ſame manner were not diſſolved.

VII. The mode hitherto practiſed of uſing tubes open at both ends, at which the gaſtric fluid was certainly at liberty to enter, is that of Reaumur. But this fluid having no other acceſs, cannot exert its action on the incloſed grains ſo powerfully as when they are looſe in the ſtomach, as Reaumur ingenuouſly confeſſes. To obviate this incovenience in ſome meaſure, I had the ſides perforated with a great number of holes. I had moreover recourſe to another expedient. I employed hollow globules of braſs half an inch in diameter, and pierced like a ſieve, which I could open and ſhut at pleaſure by means of a ſcrew worked upon the edge of the two hemiſpheres, into which each globule was diviſible. With theſe new tubes and ſpherules I repeated the preceding experiments, not only upon common fowls, but upon ducks, turkeys, geeſe, doves, and pigeons: and as a larger quantity of liquor could now find its way to the incloſed ſubſtances, they were more thoroughly ſoaked, and had acquired a bitterer taſte (III); but I could never perceive the ſlighteſt token of ſolution, though they continued a long time in the ſtomach.

VIII. Theſe facts furniſh an irrefragable proof, that the trituration of ſeeds in the ſtomach of granivorous birds, is ſolely owing to ſtrong preſſure and repeated and violent percuſſions: effects produced by the powerful muſcles with which that organ is provided.

IX. The contents of the ſtomach are ſo violently agitated as to be driven in at the open ends and through the holes of the tubes and ſpherules, which occaſions ſome confuſion. Hence I have frequently found it of ſervice to introduce theſe receivers when the ſtomach is empty, and to keep the animal faſting during the whole time of the experiment.

X. The ſtrong percuſſion of the ſtomach renders another precaution highly neceſſary. The thickneſs of the tubes and ſpherules ſhould be conſiderable, otherwiſe the obſerver, when he takes them out of the ſtomach, will find them broken, cruſhed, or diſtorted in a moſt ſingular manner, if they have been long retained. Reaumur mentions ſeveral accidents of this ſort (*a*); and I have ſeen inſtances

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(*a*) In the Memoir quoted above.

without number of ſuch contuſions, one of which I cannot forbear relating here. Having found that the tin tubes which I uſed for common fowls were incapable of reſiſting the ſtomach of turkeys, and not happening at that time to be provided with any tin plate of greater thickneſs, I tried to ſtrengthen them by ſoldering to the ends two circular plates of the ſame metal, perforated only with a few holes for the admiſſion of the gaſtric fluid. But this contrivance was ineffectual; for after the tubes had been twenty hours in the ſtomach of a turkey, the circular plates were driven in, and ſome of the tubes were broken; ſome compreſſed, and ſome diſtorted in the moſt irregular manner.

XI. I then tried the following means of preventing this inconvenience. Having perforated the circular laminæ in the center, I paſſed an iron wire through the holes, and bound it tight round the outſide of the tubes; when the two ends met, they were twiſted together. And now though the ſoldering ſhould be deſtroyed, yet this contrivance would prevent the circular laminæ from receding from the ends of the tube, unleſs the wire paſſing through them ſhould be broken. I prepared four tubes in this manner, and gave them to a turkey ſix months old. After they had remained a whole day in the ſtomach, I killed the animal; and my aſtoniſhment was extreme at finding that the tubes, in ſpite of my expedient, were very much damaged. All the iron wires were broken, two where they were twiſted, and the two others at their entrance into the tubes: the laminæ, ſo far from remaining ſoldered to the tubes, were found amongſt the food; they were not flat as at firſt, but ſome were bent ſo as to form an angle, ſome curved, and in others, one part was preſſed cloſe to the other. The tubes had ſuſtained equal injury; two of them were flattened as if they had been ſtruck by an hammer, the third was moulded into the ſhape of a gutter, the ſoldering of the fourth was deſtroyed, and it was expanded like a wafer.

XII. Theſe phænomena will leſs ſurprize thoſe who have learned from Redi (*a*) and Magalotti (*b*), how ducks, fowls, and pigeons pulverize hollow globules of glaſs in a very ſhort ſpace, and even ſolid ones in a few weeks. I have already obſerved, that I repeated theſe experiments with the greateſt ſucceſs (*c*). Some ſpherules of glaſs blown by the lamp, and ſo thick that they would ſeldom break when thrown on the ground, were generally reduced to ſmall fragments, after remaining three hours in the ſtomachs of hens and capons; the fragments were not ſharp as when they are broken by the efforts of the hand, but as obtuſe as if their edges and points had been abraded by a grinding-ſtone. The longer the ſpherules continued in the ſtomach, the more minutely were they triturated; ſo that in a few hours they were reduced to a maſs of particles, not larger than grains of ſand. The rapidity alſo of this proceſs appears in ſome meaſure proportional to the ſize of the animal. A wood-pi-

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(*a*) Eſperienze intorno a coſe naturali.

(*b*) Saggio di naturali eſperienze.

(*c*) In the Introduction.

geon generally breaks them leſs ſpeedily than a chicken, a chicken than a capon, but a gooſe the ſooneſt of all. The reaſon is plain, ſince the larger ſpecies have thicker and more powerful ſtomachs.

XIII. From theſe and other facts which I ſhall adduce hereafter, we may collect how much the celebrated Pozzi, formerly Profeſſor at Bologna, was miſtaken, when he conſidered the obſervations (*a*) of the Florentine Academicians, and of Redi on the power of certain animals to reduce globules of glaſs to pieces as falſe, becauſe he failed in his attempts to repeat their experiments on pigeons. Let me here introduce an incidental remark. It is the cuſtom of certain dabblers in philoſophy to deny facts, however particularly deſcribed, and though related by perſons of the higheſt authority, merely becauſe their own endeavours fail of ſucceſs. But they do not reflect, that this is acting in direct oppoſition to the principles of ſound logic, by which we are taught that a thouſand negative cannot deſtroy a ſingle poſitive fact, ſince it is ſo very eaſy to omit ſome one or other of the many circumſtances requiſite to the ſucceſs of an experiment. The Bolognian Phyſician has fallen into this error; inſtead of ſo raſhly inferring from his own obſervation the falſity of the contrary event, he ought to have multiplied and varied his experiments; and if he had done this with proper precautions, he would have confirmed, inſtead of contradicting the relation of the Florentine philoſo-

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(*a*) In his ſhort anatomical eſſay printed at Bolognia by Lælius a Vulpe.

phers. We muſt ſuppoſe, that the ſtomachs of his pigeons were too weak and flaccid to abrade and break ſubſtances of ſuch hardneſs as glaſs, from their being in an unhealthy ſtate or too young; for in theſe caſes they are by no means capable of producing ſuch effects, as I have found from actual experience.

XIV. My celebrated countryman Vallіſneri, in his judicious *anatomy of the oſtrich*, proves that the hardeſt ſubſtances, ſuch as ſtones, wood, glaſs, and even iron itſelf, are reduced to pieces in the ſtomach of this enormous bird by a ſolvent; he alſo inclines to think, that glaſs is attacked and broken by a ſimilar liquor, which he imagines to exiſt in the ſtomachs of fowls, without the concurrence of muſcular action. But the hypotheſis of Vallіſneri is evidently groundleſs, for ſeeds, as we have ſeen above, remain unaltered whenever they are defended by tubes. And when pigeons, fowls, or turkeys, are forced to ſwallow ſeveral balls of glaſs at once, ſome incloſed in tubes, and others naked, the latter are reduced to ſmall fragments as uſual, while the former remain entire. That the gaſtric muſcles are the ſole cauſe of this effect, will appear ſtill more evidently from facts to be related in the ſequel (XV).

XV. Before I proceed farther in the recital of experiments immediately relating to digeſtion, it may be proper to mention ſome other phænomena analogous to thoſe juſt deſcribed. They may help to convey more diſtinct notions concerning this function in animals with gizzards; the ſmooth and blunt ſubſtances hitherto employed, could not injure the ſtomach.

mach. It was therefore an object of curioſity to enquire what would happen when ſharp bodies were introduced. It is well known how readily broken glaſs will lacerate fleſh. I gave a cock ſeveral fragments of a broken pane, each about the ſize of a pea; they were wrapped up in paper, to prevent the œſophagus from being torn as they paſſed through it. I was well aſſured that this cover would be immediately deſtroyed on its entrance into the ſtomach, and leave the glaſs at liberty to act with its points and edges. The animal was killed in twenty-four hours, and the glaſs was found in the ſtomach; but on this, as well as former occaſions, the angles were ſo far obliterated, that upon putting ſome of the fragments on the palm of one hand, and rubbing them forcibly on the back of the other, I did not receive the leaſt hurt. Upon weighing the glaſs, it appeared to have loſt twenty-four grains; nor was it difficult to diſcover what was become of the miſſing particles, for the ſides of the ſtomach, when viewed attentively, glittered with innumerable vitreous points. On the contrary, ſome broken bits of glaſs, that were incloſed in two tubes, of which one was given to a hen, and the other to a turkey, and left twenty-four hours in the ſtomachs, were not at all abraded at their points or edges.

XVI. Similar pieces of glaſs, retained two days in the ſtomach of a wood-pigeon, gave me an opportunity of obſerving other remarkable fractures and abraſions. As I have mentioned this bird, I will relate a fact very applicable to our preſent purpoſe. I gave a wood pigeon an unpoliſhed twelve-ſided garnet,

net, of the ſize of a moderate nut, with the intention of inſpecting the ſtomach in a few hours afterwards; the bird was confined in a cage, but made its eſcape by an unforeſeen accident, and was confounded with many others kept in another place, ſo that I was not able to diſtinguiſh it; it did not fall into my hands for a month. The garnet, which had remained all this time in the ſtomach, filled almoſt its whole capacity; a circumſtance which a little ſurprized me, ſince it had taken its food, and been nouriſhed very well. But I was more ſurprized at finding the angles of this hard ſtone blunted in ſome places.

XVII. But the reader is impatient to know what injury the ſtomach received from the retention of theſe ſharp bodies, and the violent agitation they muſt have undergone during the abraſion of their moſt pointed parts. To ſatisfy my own curioſity, as well as that of others, I opened the cock and two wood-pigeons (XV, XVI), and examined the internal coat of the ſtomach with the cloſeſt attention, after having waſhed away the contents. I moreover diſſected it away from the nervous coat; this was eaſily effected: and I could now examine it to greater advantage, but notwithſtanding all my pains, found it perfectly entire. No laceration, no diviſion, not the ſmalleſt jagged appearance; it was in every reſpect like ſtomachs that had not afforded reception to any unuſual ſubſtance. Only the coat of that ſtomach which had retained the large garnet for a month, was about three times as thick as it commonly is.

XVIII. Finding that theſe fowls ſuſtained theſe experiments unhurt, I ſubjected them to two others far more dangerous. Twelve ſtrong tin needles were firmly fixed in a ball of lead, the points projecting about a quarter of an inch from the ſurface. Thus armed, it was covered with a caſe of paper, and forced down the throat of a turkey. The bird retained it for a day and half without ſhewing the leaſt ſymptom of uneaſineſs. Why the ſtomach ſhould have received no injury from ſo horrid an inſtrument, I cannot explain: the points of the twelve needles were broken off cloſe to the ſurface of the ball, except two or three of which the ſtumps projected a little higher. The ball had not loſt its general ſhape, but was marked with ſeveral indentations, that certainly were not upon it at firſt. Two of the points of the needles were found among the food, the other ten I could not diſcover either in the ſtomach or the long tract of the inteſtines; and therefore concluded that they had paſſed out at the vent.

XIX. The ſecond experiment, ſtill more cruel, conſiſted in fixing twelve ſmall lancets, very ſharp both at the points and edges, in a ſimilar ball of lead. They were ſuch as I uſe for the diſſection of ſmall animals. The ball was given to a turkey cock, and left eighteen hours in the ſtomach; at the expiration of which time that organ was opened, but nothing appeared except the naked ball, the twelve lancets having been broken to pieces; I diſcovered three in the large inteſtines, pointleſs and mixed with the excrements; the other nine were miſſing, and had probably been voided at the vent.

The ſtomach was as ſound and entire as that which had received the needles.

XX. Two capons, of which one was ſubjected to the experiment with the needles, and the other with the lancets, ſuſtained them equally well. My next wiſh was to know how much time elapſed before the beginning of the fractures. By repeated experiments on turkeys that were killed after intervals ſucceſſively ſhorter, I found that theſe ſharp bodies begin to be broken and loſe their ſhape in about two hours. This at leaſt happened in two individuals of the ſpecies: in one four of the lancets, and in the other three of the needles were broken within that ſpace; the others were blunted, but continued fixed in the balls.

XXI. Let it not however be ſuppoſed, that the ſtomach in this claſs of birds is never vulnerable by ſharp ſubſtances. In pullets it certainly is ſometimes very much injured. I obliged two pullets to ſwallow ſome pins from which the heads had been taken. One was killed in eight, and the other in thirty-two hours. The former had not at all ſuffered, but two pins were implanted in the ſtomach of the latter. Theſe ſtomachs, as well as thoſe of many other animals, are full of rugoſities, in one of which the two pins were fixed almoſt perpendicularly, one to the depth of a line, and the other to that of three lines: they were oppoſite to the moſt muſcular part of the organ. Some force was required to extract them; at the puncture appeared a little clotted blood, with an evident livid colour around.

XXII. But whatever concluſion we are to draw from this laſt fact, it is certain, that the ſtomach of ſuch birds is in general not ſubject to any injury from the introduction, reſidence, or trituration of theſe and the like ſubſtances, as I have learned from a vaſt variety of experiments. But how is it poſſible, ſome will enquire, that the gaſtric muſcles can contuſe, triturate, and even ſometimes reduce to impalpable powder (as when glaſs is employed, XII, XIV, XV, XVI) theſe pointed bodies without injury to themſelves? If the muſcles act with ſo much force, muſt not the ſubſtances neceſſarily re-act upon the muſcles? And will not this re-action cauſe the laceration of the internal coat of the ſtomach, which, though it is indeed very firm and compact, cannot ſuſtain ſuch violent ſhocks with impunity?

XXIII. This objection was immediately ſtarted, upon the diſcovery of the wonderful force with which digeſtion in poultry is effected, and an attempt was made to remove it in the following ingenious manner. It had been long known, that fowls, and other birds of the ſame claſs, have always a ſmaller or larger ſupply of little pebbles in their ſtomachs. It was therefore conceived, that theſe pebbles ſerve as a ſhield to the muſcles. Hence it follows, that the comminution of bodies forced into the ſtomach is the immediate effect of the pebbles, and only the mediate effect of muſcular action. Accordingly, the Academicians of Cimento have obſerved, that thoſe ducks and fowls that contain moſt ſtones in their ſtomachs, ſooneſt reduce ſpherules of glaſs to powder. Redi thinks, that the ſtones perform the office of teeth (*a*); and Reaumur ſuppoſes them neceſſary to digeſtion (*b*).

XXIV. In the courſe of my numerous obſervations I can ſafely aſſert, that I never opened the ſtomach of a pigeon, turtle-dove, dove, partridge, fowl, turkey, gooſe, &c. without finding ſmall ſtones within it. I have alſo found what is remarked by Reaumur, that the ſize of the ſtones is apparently proportional to the ſize of the bird. They are generally of a roundiſh ſhape, whether they acquire it from friction within the cavity of the ſtomach, or have it before they are ſwallowed. They are commonly bits of quartz, ſometimes mixed with calcareous fragments. In the ſtomach of a turkey hen I have counted above 200, and above 1000 in that of a gooſe. Their exiſtence is therefore indubitable. But is it equally certain, that they are the immediate inſtruments of trituration? He who is unprejudiced in favour of any theory muſt immediately perceive, that this is a mere hypotheſis, convenient indeed and plauſible, but requiring to be confirmed by experiment.

XXV. To this teſt I have endeavoured to bring it, and would willingly hope that I have decided the queſtion. According to the obſervation of the Academicians, the birds that contain moſt ſtones, ſooneſt triturate hard ſubſtances. Nothing was more eaſy than to repeat the experiment. This I did upon ducks and fowls, the two ſpecies mentioned by thoſe learned writers, ſometimes obliging them to

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(*a*) L. c.

(*b*) Mem. cit.

ſwallow globules of glaſs, ſometimes ſmall thin tubes of tin, and at others ſeeds defended by a ſtrong cover, ſuch as nuts of a moderate ſize. It was neceſſary that all circumſtances ſhould be alike, that the birds ſhould be of the ſame ſpecies and age, and of equal vigour, &c. Not to weary the reader with too minute a detail, I ſhall only mention the reſults. In a hen and two ducks, not abundantly ſupplied with pebbles, the injury ſuſtained by the ſubſtances was not ſo great as in three other like fowls more amply provided with them. But in four hens the effect produced was exactly the ſame, as far as I could judge, though the ſtomachs of three were leſs copiouſly furniſhed than that of the fourth.

XXVI. Having collected a large quantity of ſtones from the diſſection of many gizzards, I thought they might be uſeful in the preſent inquiry; I therefore gave a certain number to ſome fowls and ducks, while others were left with thoſe which they had ſwallowed ſpontaneouſly. The former, according to the obſervation of the Florentine Academicians, ought to break hard ſubſtances ſooner than the latter. And ſo indeed it ſometimes happened, but at others the event was different. Wherefore not being able to aſcertain the object of my enquiry by theſe experiments, I had recourſe to other means of ſolving the problem.

XXVII. The moſt deciſive mode of determining the uſe of ſtones in digeſtion, evidently was to take them away altogether, either by expelling thoſe already ſwallowed, or by preventing the admiſſion of any at all. To evacuate thoſe already accumulated, it was neceſſary to confine the birds in cages where they could not find freſh ones, and it might be hoped, that the old ones would be gradually voided with the excrements. Accordingly, ſeveral fowls, turkies, turtle-doves, and ducks were confined ſeparately, and that all ſuſpicion of their picking up pebbles might be removed, the cages were raiſed to ſuch an height that they could not reach the floor with their beaks. The bottoms were made of oſiers placed at a diſtance from each other, that if the ſtones ſhould paſs out with the excrement they might not remain in the cage, and be ſwallowed again, but fall to the ground. I fed them myſelf the whole time, taking care that the food, conſiſting of corn, vetches, and maize, was free from every foreign matter, ſo that I was certain not a ſingle grain of ſand or the ſmalleſt ſtone was ſwallowed by them.

XXVIII. In the courſe of a few days I perceived ſome ſtones among the excrement, and they continued to be voided during the time of confinement. Two days before the end of the month, when they were to be killed, I forced ſome to ſwallow tubes of tin, others glaſs globules, ſome naked and ſome armed with needles and lancets (XVIII, XIX, XX.). I likewiſe gave them ſome grains of wheat, but did not allow them to undergo the natural proceſs of maceration in the crop. On the 30th day every ſtomach was carefully examined, and though I did not completely attain the end in view, yet I gained conſiderable information on the ſubject. Not a ſingle ſtomach indeed was free from ſtones, but they were few in number, in ſome inſtances not amounting to above four or five, and thoſe very ſmall. The contuſions, however, on the tin tubes, the indentations on the naked balls, the fracture of the needles and lancets, the trituration of the grain had alike taken place in every ſtomach; nor did it appear, that the diminution of the quantity of ſtones at all contributed to diminiſh the alteration of the ſeveral ſubſtances, or to occaſion any injury to the organ that contained them. And leſt it ſhould be objected, that theſe hard bodies themſelves performed the office of pebbles by rubbing violently againſt each other in conſequence of the action of the gaſtric muſcles (an objection manifeſtly trivial) I had taken care that each bird ſhould not have more than one tin tube, or one glaſs globule, &c. Theſe ſolitary ſubſtances were bruiſed or broken juſt as effectually as when many were put into one ſtomach; and this viſcus remained as free from injury.

XXIX. Though theſe facts abundantly prove, that trituration does not depend on the ſtones ſwallowed by the birds in queſtion, but upon the ſtrength and action of the gaſtric muſcles, I yet wiſhed for proofs more deciſive, from obſerving what happens in ſtomachs that have not received any ſtones. The judicious reader perceives at once, that to accompliſh my purpoſe, it was neceſſary to procure young neſtlings that had never been in queſt of food. Accordingly, ſome wood-pigeons, of which the feathers had not begun to ſhoot, were brought me; but I was diſappointed in my expectations, for even their tender ſtomachs were not free from pebbles, which doubtleſs were mixed with the food carried to them by their parents. Three of theſe young birds were ſacrificed to my curioſity. The ſtomach of the firſt contained eight ſtones, of the ſecond eleven, of the third fifteen; together they weighed thirty-two grains, and conſiſted chiefly of quartz.

XXX. As theſe experiments did not anſwer my purpoſe, it was neceſſary to take up the enquiry at an earlier period, and employ birds leſs advanced in growth; nay, for greater certainty ſuch as were juſt quitting the egg, and therefore could not have received food from their parents. The ſtomach, it is obvious, could not contain ſtones of any kind. I was at the pains of keeping ſeveral neſtlings in a warm place, while they remained unfledged, and feeding them till they were able to peck. They were then confined in a cage, and ſupplied at firſt with vetches ſoaked in warm water, and afterwards in a dry and hard ſtate. In a month after they had begun to peck, hard bodies, ſuch as tin tubes, glaſs globules, and fragments of broken glaſs were introduced with the food; care was taken that each wood-pigeon ſhould ſwallow only one of theſe ſubſtances. In two days afterwards they were killed. Not one of the ſtomachs contained a ſingle pebble, and yet the tubes were bruiſed and flattened, and the ſpherules and bits of glaſs blunted and broken: this happened alike to each body, nor did the ſmalleſt laceration appear on the coats of the ſtomach.

XXXI. I did not confine my obſervations to this one ſpecies. With the ſame view I ſet under a turkey-hen ſeveral eggs, partly her own, and partly of a common hen. When the chickens were hatched I took charge of them myſelf, and employed the ſame precautions as with the wood-pigeons (XXX). They were confined for fifty-five days in ſeparate cages, and their food conſiſted of various ſorts of grain. The laſt days they had to live, I forced them to ſwallow the hard indigeſtible ſubſtances ſo often mentioned. Upon examination, the ſtomachs appeared to be free from ſtones, yet the fragments and ſpherules of glaſs, and the tin tubes, were not on this account either the leſs or the more bruiſed or broken. Hence then we have at length a deciſion of the famous queſtion concerning the uſe of theſe pebbles, ſo long agitated by authors. It appears, that they are not at all neceſſary to the trituration of the firmeſt food, or the hardeſt foreign ſubſtance, contrary to the opinion of many anatomiſts and phyſiologiſts, as well ancient as modern; I will not, however, deny, that when put in motion by the gaſtric muſcles, they are capable of producing ſome effect on the contents of the ſtomach.

XXXII. But for what purpoſe are they deſigned? If they are not neceſſary to the trituration of the food, are we to ſuppoſe that they contribute in any other way to digeſtion? Do they create a keener appetite, or maintain a better ſtate of health, as ſome conceive? Are they found in the ſtomach becauſe they are caſually mixed with, and as it were concealed by, the food; or, becauſe they are deſignedly ſwallowed, and even ſought after?

The firſt queſtions are already anſwered, or rather precluded, ſince we have found, that birds unprovided with pebbles take their food, are nouriſhed and grow juſt as well, and are as briſk and lively as others abounding with them; an obſervation I have made with great ſatisfaction upon young pigeons, turkies, and chickens reared in the manner deſcribed above (XXX, XXXI).

XXXIII. The laſt queſtion will be readily ſolved, if grown up chickens take their food in the ſame way as young ones; for theſe ſwallow every thing that comes in their way. I have often ſcattered amongſt them various ſubſtances unfit for their nouriſhment, ſuch as pebbles, bits of brick, chalk, or rubbiſh, which they pecked with eagerneſs, whether their ſtomach was full or empty. One day I threw before them a large quantity of the ſhells of the little fiſh, termed *Lice* by Conchologiſts, which they devoured till their crop was full, juſt as if it had been the moſt agreeable food. If they retain the ſame diſpoſition when full grown, we may reaſonably conclude, that the collecting of pebbles is leſs the effect of choice than ſtupidity; as the oſtrich, according to Valliſneri and Buffon, devours without diſtinction whatever comes in its way, ſticks, and ſtones, and cords, and glaſs, and metals, &c. ſuch is its dulneſs, and ſo obtuſe its organ of taſte (*a*). But fowls grown to their full ſize, and when their natural inſtinct, that lay dormant while they were young, comes to be unfolded, change their manners in this as well as many other reſpects. A capon confined in a cage by Redi, died of hunger ſooner than it would ſwallow pebbles offered to it in place of

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(*a*) Buffon Hiſt. des Oiſeaux. T. 2. Ed. in 12. Valliſn. Op. in fol. T. 1.

food (*a*). With me too three hens and one turkey, kept ſeparately, died in the courſe of a few days, when inſtead of giving them food, I ſcattered before them a quantity of ſmall ſtones. After their death the number of the ſtones was the ſame, though they would appear to be of the moſt proper kind, having been taken from the ſtomachs of other individuals belonging to the ſame ſpecies. When pebbles are mixed with the food, I have obſerved, that poultry, eſpecially when hungry, pick them up. I ſhould then incline to believe, that the ſtomachs of theſe birds generally contain a quantity of ſmall ſtones; not becauſe they are ſought for and ſelected by deſign, as many ſuppoſe, but becauſe they frequently happen to be mixed with the food.

XXXIV. Having ſhewn that the pebbles are not the cauſe of trituration, we muſt conclude, that it is the ſole and immediate effect of the gaſtric muſcles, which, as it is well known, are very ſtrong, and compoſed of firm and compact layers, and muſt, therefore, when ſet in motion, act with great force. To be more fully ſatisfied of this, let the ſtomach of a dog, ſheep, or a man be compared with the gizzard of a duck, turkey, or gooſe; we ſhall then perceive the enormous difference between the thick muſcular coat of the one, and the thin one of the other.

XXXV. The internal coat, or that which immediately lines the cavity of the ſtomach, deſerves particular attention. In many animals, and in man himſelf, it is ſoft and villous; but in gallinaceous birds it is hard and

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(*a*) Degli Anim. viventi negli Anim. viventi.

cartilaginous. When ſeparated from the next, which anatomiſts call the nervous coat, it ſoon becomes dry and very hard. In turkies and geeſe, in which it is thicker and ſtronger than in common fowls, I have often diſſected it away, and ſpreading it upon a table, have drawn along it lancets, needles, bits of glaſs, and ſuch ſharp ſubſtances as are triturated in the ſtomach without any perceptible injury to it. If indeed my hand preſſed with conſiderable force, thoſe parts, over which the keen bodies paſſed, were diſunited, whether it was ſeparated or adhered to the other coats.

XXXVI. But theſe ſubſtances may act in a quite different manner when under the direction of the hand, than when ſet in motion by the gaſtric muſcles, and when the internal coat is not extended but forms a cavity, as it does when the ſtomach is entire. I therefore wiſhed to know what happens to ſubſtances incloſed in the ſtomach ſeparated from the animal, and preſſed externally with the palms of both hands, and agitated in various directions. The ſtomach of a turkey hen was firſt cleared of its contents by forcing them out through the pylorus, and then a large quantity of ſharp pieces of glaſs were introduced, which were kept in motion for a quarter of an hour by preſſure and percuſſion on the outſide of the ſtomach. . I was in hopes, that I ſhould thus, in ſome meaſure, imitate the natural motion. Nor was the expedient altogether ineffectual; for the internal coat was only perforated with two little holes, ſuch as the point of a needle would have made, and yet part of the glaſs was reduced to powder, and part had loſt its ſharp edges. Different effects then are produced, when this coat is ſubmitted to experiment after it has been removed from its natural ſituation, and when it adheres to the others. Nevertheleſs I am willing to allow, that how it ſhould be capable of blunting and breaking the keeneſt bodies without ſuſtaining any injury itſelf, ſtill continues a matter of great ſurprize.

XXXVII. But if the inſide of the gizzard be certainly agitated ſo violently during the trituration of the food, will not the motion be perceptible on the outſide? Reaumur, induced probably by this reflection, laid open the abdomen of ſome of the fowls in queſtion, and watched the ſtomach, but could not perceive what he perhaps imagined took place. They always ſeemed perfectly at reſt, except the gizzard of a capon, which contracted and dilated alternately; he moreover ſaw certain fleſhy cords moving in an undulating direction, but very ſlowly and gradually (*a*).

XXXVIII. I have perceived ſimilar motions in two turkey cocks. Upon preſſing upon the ſtomach forcibly with my hand, I felt a ſlight pulſation that produced a ſenſation of creeping, but was ſoon aware, that this was owing only to the beating of numerous little arteries, which run upon the ſurface of the viſcus. When a perforation is made in the heart of a living animal, and a finger introduced through it, it is well known that ſtrong preſſure is felt at the time of its contraction. I made

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(*a*) Mem. cit.

this experiment upon the gizzard of a duck, but was not ſenſible of the ſlighteſt compreſſion.

Conceiving that the ſtomach muſt exert its principal action when it is irritated by ſubſtances filling its cavity, I introduced ſome nuts into the gizzard of a turkey hen, that had been kept faſting for a day. During the whole time I watched it attentively, through an opening made in the abdomen; when it had received only a few nuts it ſhewed no ſign of motion, but when it was nearly full it ſwelled violently, and then collapſed again of a ſudden. Theſe alternations were ſometimes general, and at others confined to a narrow ſpace; they did not continue ten minutes, probably becauſe the death of the animal was approaching from the aperture of the abdomen. The nuts were unbroken, but evident contuſions appeared upon the ſurface. This diſtinct view of the motions of the ſtomach I aſcribe to unuſual good fortune, ſince, with the exception of one only other turkey, the ſtomachs of many birds of the ſame claſs remained at perfect reſt, after they had been filled in the ſame manner. If however we conſider the very morbid ſtate of the animal when the abdomen is laid open, we ſhall not be much ſurprized at this phænomenon.

XXXIX. The various facts related in the preceding paragraphs irrefragably prove, that the food of ducks, fowls, geeſe, partridges, &c. muſt undergo the mechanical action of the gaſtric muſcles, before it can be broken down, and reduced to an impalpable pulp. But are we to ſuppoſe, that digeſtion depends on this action, and that ſimple trituration converts the aliment into that pultaceous maſs denominated *Chyme?* Or rather, that this maſs is generated by means of juices either prepared or collected in the ſtomach; and that trituration is a co-operating, but not the immediate cauſe of digeſtion? I imagined that the tubes and ſpherules, which had already afforded me ſo much information, would not now be without their uſe. If the gaſtric juices convert into chyme the food which trituration has prepared for digeſtion, let ſome food ſo prepared be incloſed in the tubes and ſpherules, and let us ſee whether it will be diſſolved according to this hypotheſis; for it muſt be thoroughly ſoaked in theſe juices. I firſt filled a tube and ſpherule with crumb of wheaten bread maſticated, and introduced them into the gizzard of a hen. In twenty-three hours they were taken out, and the quantity of bread was much diminiſhed, eſpecially at the ends of the tube, where it was alſo ſofter than at firſt, and had acquired a bitter taſte. The ſame tube and ſpherule were forced into the gizzard of another hen, where they remained fourteen hours; after which there was no appearance of bread in either of the receivers.

XL. I repeated the experiment upon a third hen, with bread of maize inſtead of wheat; the tube and ſphere were emptied in a day and half. As there was here no trituration nor any other power, except the action of the gaſtric fluid, it ſeemed reaſonable to conclude, that this fluid had diſſolved and converted the bread into chyme, and ſo enabled it to paſs through the holes in the receivers. A doubt however ſuggeſted itſelf, and kept me in ſuſpenſe;

penſe; without ſuppoſing the tranſmutation of the bread into chyme, the gaſtric fluid by merely diluting it, like water, might render it capable of paſſing out of the tubes and ſpherules.

XLI. A ſubſtance not ſoluble by ſimple maceration, and at the ſame time ſofter than grain, upon which the gaſtric juices have no action (III, IV, V, VI, VII.), was wanting to clear up the doubt. Fleſh ſeemed to correſpond to this deſcription. Fleſh is digeſted by many birds with gizzards, which for the moſt part are both frugivorous and granivorous; I therefore filled four tubes with ſome veal (*a*) bruiſed very ſmall to ſupply the want of trituration, and forced them into the ſtomach of a hen. They were taken out in twenty-four hours, and the fleſh was in the following ſtate: In the tube that came firſt to my hands it did not amount to above one-twentieth of its original bulk, in two others it had ſuffered nearly the ſame diminution; the only difference appeared in the fourth, which was not open at both extremities like the other three, but cloſed at one end with a circular plate of iron. The fleſh contiguous to the plate preſerved its red colour and natural conſiſtence, and did not ſeem at all waſted; but at the open end it was reduced to two thirds of the length of the tube, of which it had at firſt occupied the whole; the part that continued firm and red retained the true flavour of fleſh; at the oppoſite end it had entirely loſt that flavour, and the ſurface, to the depth

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(*a*) Wherever I mention fleſh without an epithet, I mean raw fleſh.

of a full line, was beſides reduced to a pulp, and had acquired a cineritious colour: The inconſiderable reſiduums in the other tubes were altered in the ſame manner.

The immediate conſequences of theſe experiments are ſelf-evident. The remarkable diminution of the fleſh aroſe from its having been in great meaſure diſſolved and digeſted; for all phyſiologiſts agree in conſidering the change of colour and taſte, and the tranſmutation of the food to a pultaceous maſs in the ſtomach, as the characteriſtic marks of digeſtion. The three tubes, of which the ſides were perforated and the ends open, admitted the gaſtric liquor at every part. Hence the remarkable waſte of the fleſh in them. The caſe was different in the tube cloſed at one extremity, and nothing can be more obvious than the reaſon; for as the liquor could only enter at one end, it could only there diſſolve the fleſh.

XLII. This experiment deciſively proves, that the gaſtric liquor was the cauſe of digeſtion in the preſent inſtance; and it was eaſy to foreſee, that others upon the ſame claſs of birds would be attended with the ſame reſult. Some tubes filled with fleſh were next introduced into the gizzard of a very large turkey cock, but the lattice work at the open ends, though it conſiſted of iron, could ill withſtand the action of ſuch powerful muſcles. Upon examination ſeven hours afterwards, it was found ſeparated from the tubes, and coiled up in one maſs near the pylorus, in the midſt of the pebbles and ſcoriæ of the food, ſome of which were jammed ſo cloſely in the tubes, that there was ſome difficulty in forcing them out with the point of a penknife. I could not perceive the ſmalleſt fragment of fleſh amongſt them, and remained in doubt whether it had been digeſted, or expelled by theſe extraneous bodies. I reſolved to ſubmit this ſpecies of bird to further experiments, but was obliged to abandon the tubes, and have recourſe to the hollow ſpherules, of which I have ſpoken above (VII). They were made thick and ſtrong, with many ſmall pores over the whole ſurface, in order to obviate two inconveniencies, the one leſt the receivers ſhould be unable to reſiſt the violent impulſes of the ſtomach, the other to prevent the matters compreſſed and agitated by the action of the muſcles from entering ſo readily into them. Two of theſe ſpheres were given to a turkey cock eleven months old, and in twenty-four hours extracted from the gizzard. They contained at firſt about twenty-eight grains each of beef and veal bruiſed very ſmall. Upon opening them after the ſame interval as before, and weighing the fleſh, the beef was found to have loſt nine, and the veal thirteen grains. I muſt not however omit to remark, that they were both fully impregnated with gaſtric liquor, and conſequently would have weighed ſtill leſs if they had been free from it. The beef and veal, when touched with the point of a knife, ſeemed tenderer than in their natural ſtate, and reſembled a ſoft paſte rather than fleſh. They had the bitter taſte of the gaſtric juice with which they were impregnated, and the colour approached more to white than red. They were replaced in the ſpheres, and kept twelve hours in the gizzard of another turkey-cock.

Upon a freſh examination, the beef weighed only eight, and the veal only five grains. The gaſtric fluid had therefore produced a new ſolution, and this proceſs was entirely completed after the ſpheres, into which the fleſh was put for the third time, had continued five hours in the ſtomach of a third turkey-cock.

XLIII. Fleſh is digeſted by the gaſtric liquor of geeſe as well as of turkies. Eleven grains of beef, incloſed in a ſpherule, were entirely diſſolved in two days in the gizzard of one of theſe large birds.

I will not deſcribe three other reſults obtained, one from an hen, and the two others from two capons; ſince, with reſpect to the digeſtion of the fleſh, they are exactly like thoſe juſt mentioned.

All theſe experiments were made with fleſh bruiſed very ſmall; this condition is not indeed indiſpenſably requiſite, but it very much promotes digeſtion. The bruiſed fleſh was always diſſolved in two days, but when entire that proceſs was not completed in four, and ſometimes not even in five days. The reaſon of this difference is obvious. The more fleſh is bruiſed, the larger ſurface does it acquire; and in proportion to the increaſe of ſurface, more points are expoſed to the action of the gaſtric liquor, which will conſequently ſooner complete the ſolution.

XLIV. Before I proceed further and conclude the preſent diſſertation, I muſt notice an experiment of Reaumur, which does not perfectly agree with thoſe juſt related. The greateſt part of his memoir is employed in ſhewing the great force of the gizzard of gallinaceous fowls in triturating the food; in the remainder he endeavours to prove, that this viſcus contains no menſtruum of ſufficient efficacy to produce ſolution. In ſupport of this propoſition, beſides the argument derived from barley continuing unaltered within the tubes, he adduces the following fact, which requires to be particularly related. It is well known, how greedily ducks devour, and how ſoon they digeſt, fleſh. In order therefore to obtain the information he wanted, Reaumur had recourſe to this bird. Having provided ſix tubes, four of lead, and two of tin, he incloſed in the former bits of veal of the ſize of a grain of barley, and in the latter ſome conſiderably larger. Theſe ſix tubes he gave to a duck at different times; viz. a leaden one at ten o'clock in the morning, and another at eight in the evening; next day a third was given at ſix in the morning, together with the two tin tubes; laſtly, at nine the ſame morning the animal was made to ſwallow the laſt leaden tube, and at ten was killed. Of the four leaden tubes, one was voided the preceding day at nine in the evening; it was that which had been taken at ten in the morning; the other five remained in the gizzard, and the fleſh was not only entire, but as firm as at firſt. Some of the pieces retained their red colour, three of them however had loſt it. The whole capacity of ſome of the tubes was no longer filled by the fleſh; not that it had ſuffered any diminution, but becauſe it was compreſſed by the ſtones and food, which had been admitted at the open ends of the tubes. From this experiment Reaumur infers, that no menſtruum had acted on the fleſh, ſince it was not either comminuted or diſſolved. And though he does not affirm, that in the gallinaceous claſs digeſtion is the effect of trituration alone, he yet concludes, that the gizzard contains no ſolvent capable of decompoſing and digeſting the aliment.

XLV. What has been above related, ſhews how far Reaumur's concluſion ought to be extended; when we ſpeak of aliment of a hard and compact texture, ſuch as ſeeds, it muſt be allowed, that the gaſtric liquor has no action upon them (II, III, IV, V, VI, VII.); but when we are conſidering food naturally tender, like fleſh, or ſuch as is made ſo by art, like grain in the form of maſticated bread, it muſt then be allowed, that a perfect ſolution is effected by the gaſtric juices alone (XXXIX, XL, XLI, XLII, XLIII.). In Reaumur's experiment the fleſh remained ſo ſhort a time in the gizzard, that we cannot be ſurprized if it was not ſenſibly diſſolved. If we attend to the times at which he gave his tubes to the duck, and at which he killed it, we ſhall immediately perceive, that the tube which continued longeſt in the gizzard, remained in it only twenty-four hours; a ſpace inſufficient, according to my experiments on fowls, turkies, and geeſe (XLI, XLII, XLIII.), for the gaſtric liquor of theſe birds to diſſolve any ſenſible portion of fleſh incloſed in tubes. I ſhould however have condemned myſelf for a crime of omiſſion if, to the proof deducible from analogy, I had neglected to add direct experiments on ducks. Upon two ducks therefore I repeated the experiment of the French Naturaliſt, with the following variation; four tubes, each containing a bit of veal equal in ſize to a barley-corn, were given to a duck; in two of the tubes the fleſh was whole, but in the two others it had been previouſly cut into ſmall bits: in fourteen hours the gizzard was examined; the four tubes were found in it; the two entire pieces of fleſh were of their original ſize, but inclining to a white colour; the ſmall bits were alſo about the ſame ſize as at firſt, but were converted into a gelatinous paſte. The experiment was repeated upon another duck, which was not killed till the end of the ſecond day; and now the tubes that had contained the minute bits of fleſh were entirely empty; and in the others, only ſome ſlight traces of a gelatinous concocted matter remained. If we combine theſe facts with others before related, it will appear, that in the gallinaceous claſs, trituration and the gaſtric fluid mutually aſſiſt each other in performing the great function of digeſtion; the former by breaking down the aliment, acts as the pre-diſpoſing cauſe; the latter, when it is thus prepared, penetrates into it, deſtroys the texture, diſſolves the particles, and diſpoſes them to change their nature, and to become animalized.

XLVI. But what is the origin of this gaſtric fluid, ſo uſeful in digeſtion? How is it mixed with the food? And what ſucceſſive changes does the latter undergo from the action of trituration, joined to that of the gaſtric liquor? Theſe important queſtions required a ſtrict examination of the œſophagus and gizzard, as alſo of the food during its paſſage through, and continuance in, theſe parts. As experiments are more concluſive, the larger the ſcale is, on which they are conducted, I conceived that the larger ſpecies, as geeſe, turkies, ducks, and fowls, would be the beſt ſubjects for theſe enquiries. To begin then with the œſophagus of a gooſe, this canal at the end towards the mouth, has the appearance of an inflated inteſtine; it is above a foot long, and at its origin about an inch in diameter, but widens as it deſcends, for the ſpace of ſix inches and more, when it contracts like a funnel, then enlarges again, and this enlargement continues to the gizzard. The œſophagus is membranous, its ſides are ſtrong and thick; they are thickeſt at three inches diſtance from the ſtomach, on account of a fleſhy faſcia, of which I ſhall ſpeak below. If we look very attentively, we can perceive the whole œſophagus covered with points or elongated ſpots, which are moſt numerous juſt above the funnel. The faſcia appears to conſiſt of a multitude of cylindrical bodies, larger than huſked millet-ſeed. Theſe bodies ſhine through a fine membrane, externally ſurrounding the faſcia.

XLVII. If the œſophagus be inverted, and the ſpots examined by the help of a glaſs, we plainly perceive that they are follicular glands. This likewiſe is confirmed by the appearance of moiſture on the œſophagus, when they are preſſed. But the follicular glands that appear through the fleſhy faſcia like cylindrical bodies, bigger than huſked millet, as we before obſerved (XLVI), are far more eaſily diſtinguiſhable, becauſe far larger. This faſcia, which encircles the œſophagus like a ring, is above an inch in breadth, and about a line in thickneſs. Great part of it is inveſted by a thin covering of a deep yellow colour, and conſequently very liable to be torn. When this is removed, the faſcia externally appears white and rough, on account of the numberleſs prominent papillæ, each of which has an evident pore in the center. When the faſcia is ſtretched, and much more when it is preſſed between the fingers, a drop of whitiſh turbid liquor guſhes out at each pore into the œſophagus; and it may be enlarged, by continuing the dilatation or preſſure. The liquor is denſe, ſomewhat viſcid, of a ſweetiſh, and at the ſame time ſaltiſh taſte. To comprehend immediately that the pores are the excretory ducts of the follicular glands lying below, requires very ſlight anatomical knowledge: the glands appear very diſtinctly, when the membrane in which the pores are inſerted, is removed. The follicles are of a pale red colour, and full of a turbid liquor, which oozes out from the excretory ducts, when the œſophagus is kept under water.

XLVIII. Below the fleſhy faſcia, the œſophagus becomes membranous again for nearly the breadth of three quarters of an inch, when it is inſerted into the gizzard. This organ is of the ſize of the fiſt, remarkably hard, and of an irregular elliptical figure; when opened lengthwiſe at the thinneſt part, it is divided into two large muſcles, each above an inch in thickneſs, and compoſed of very compact fibres. It appears plainly, that the whole action of theſe great muſcles conſiſts in approximating with violence, and like the ſides of a vice, cruſhing and breaking to pieces all interpoſed ſubſtances. As the nervous coat adheres to theſe ſtrong muſcles,

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and as, however robuſt, it might be injured by ſuch impetuous ſhocks, nature has ſagaciouſly inveſted it with a cartilaginous coat, of a ſtructure more capable of reſiſtance, which internally lines the cavity of the gizzard.

XLIX. In turkies the œſophagus and ſtomach very nearly reſemble thoſe parts in geeſe. The former, however, is more membranous, and abounds more in follicular glands of a larger ſize, and conſequently more conſpicuous. The excretory ducts may be eaſily ſeen, and the liquor of the follicles may be readily forced out by preſſure. This liquor is tranſparent, and ſomewhat viſcid; its taſte is rather ſweet. But the œſophagus of the turkey has one peculiarity not found in the gooſe; it is provided with a burſa or bladder, well known under the name of the *crop* or *craw*. In this ſpecies it is very large. The crop at the ſides at leaſt, if not at every part, is furniſhed with follicular glands, exactly like the others. At the lower part of the œſophagus we alſo find the fleſhy faſcia, an inch in breadth, and provided with follicles much larger than thoſe of the crop or œſophagus, and in great abundance. The liquor ſeems to have the ſame properties as in the gooſe. It is viſcid, has a ſweetiſh and ſaltiſh taſte, a turbid white colour, and conſiderable denſity.

The gizzard, whether its form or the nature of its coats be conſidered, is exactly like that of the gooſe, only weaker and ſmaller in proportion to the inferior ſize of the bird.

L. I have obſerved all that has been related with reſpect to the gizzard and follicular glands of the gooſe and turkey in due proportion in the duck, common fowl, and even in ſmaller birds of the ſame claſs, as the pigeon, partridge, wood pigeon, turtle-dove, and quail; with this peculiarity only, that in the duck the œſophagus, inſtead of forming a crop, has the ſame ſtructure as in the gooſe (XLVI). I ſhall therefore omit a deſcription of theſe parts, and proceed to conſider the ſtomach in a phyſiological light.

LI. In ſpeaking of this organ, I have never mentioned either follicles or glands; for in the fowls hitherto mentioned, I could never diſcover any. The internal coat, from its cartilaginous nature, appears to be unfit for the inſertion of glandular bodies; at leaſt I was not able to find the ſmalleſt veſtige of them; nor did I ſucceed any better in the nervous or muſcular coats, notwithſtanding I examined them very narrowly. Reaumur having obſerved a vaſt number of ſhort white filaments between the cartilaginous and nervous coats, entertained ſome ſuſpicion of their being tubes or veſſels, placed there in order to diſcharge their contents into the ſtomach (*a*). I have found theſe filaments in all the gallinaceous fowls I have examined; but cannot agree with him that they remain attached to the nervous, when the cartilaginous coat is ſeparated from it: for after ſuch ſeparation, I have ever ſeen them adhere to the cartilaginous, never to the nervous coat; but any perſon may readily make the trial. Theſe

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(*a*) Mem. Cit.

filaments are very numerous; they are pointed at the extremity, oppoſite to that which is inſerted into the laſt mentioned coat, and reſemble ſhort white down, diſtinctly viſible by the naked eye in the larger birds; ſuch as the gooſe and turkey, but requiring the aid of a glaſs to be ſeen in the ſmaller ſpecies. I have divided many of various ſizes with the points of very fine needles, in order to diſcover whether they were hollow or glandular, but could never find any appearance of this kind: I have alſo ſqueezed them in order to ſee if any liquor would ooze out, but to no purpoſe; and ſo far from ſuſpecting theſe filaments of Reaumur to be vaſcular or glandular, I ſhould rather ſuppoſe them to be merely for the purpoſe of joining, or at leaſt more cloſely connecting the cartilaginous with the nervous coat.

We ſhall ſee elſewhere, that ſtomachs of the membranous kind, when they are taken out of the animal and rubbed dry, ſoon become moiſt again: this moiſture comes from inviſible veſſels and glands, diſcharging their liquor into the cavity of the ſtomach. I have made the ſame experiment on muſcular ſtomachs, but they always continued dry; the ſame thing alſo took place, when I preſſed them underneath, though this is a very effectual means of accelerating and increaſing the aqueous covering. Hence I have good grounds for ſuppoſing, that the juices found in muſcular ſtomachs do not properly belong to them, but come chiefly from the œſophagus, and in part from the duodenum, as we ſhall ſee below.

LII. Nature however, has not failed to provide the quantity neceſſary for digeſtion. We have ſeen the vaſt number of follicular glands with which the œſophagus is provided (XLVI, XLVII, XLVIII, XLIX.); they muſt needs pour in their liquor in great abundance. And experience confirms what reaſon ſuggeſts. I introduced a ſmall piece of dry ſpunge, previouſly cleanſed from every impurity, into the craw of a pigeon, in which it was left twelve hours; at the expiration of this time I opened the craw, and took it out. The ſpunge was full of liquor, and on being ſqueezed over a glaſs, afforded above an ounce. I employed larger pieces of ſpunge in fowls and turkies, and obtained more of this œſophageal liquor; the quantity in a turkey amounted to ſeven ounces in ten hours. A ſimilar liquor is procured in equal abundance, from ſuch œſophaguſes as are dilated into a large canal, inſtead of a craw, as in ducks and geeſe (XLVI, L.). This fluid is undoubtedly deſigned to ſoften the food which remains a certain time in the craw, or in the large canal; and this not only diſpoſes it to be more readily broken down, but very probably alſo communicates to it ſome quality that renders it more eaſily digeſtible. But it is likewiſe certain, as I have found from experiment, that a conſiderable part of this fluid deſcends into the ſtomach; not to mention that denſer and more viſcid liquor which diſtils from the fleſhy faſcia, lying at the bottom of the œſophagus, (XLVI, XLVII.).

LIII. Theſe various œſophageal juices acquire in the ſtomach a bitter flavour, reſembling that of the food in this viſcus: and as this taſte exactly reſembles that of bile, which in theſe animals is diſcharged through the cyſtic duct into the duodenum, I am thoroughly perſuaded that it ariſes from this ſource, in conſequence of the bile regurgitating into the cavity of the ſtomach, and being mixed with the food and œſophageal liquors collected there. I am confirmed in this perſuaſion by other facts, which I ſhall have a more convenient place for relating; not to mention the well known circumſtance of the bile being found in the ſtomach of various animals (*a*).

LIV. This collection of divers liquors in the gizzard of our fowls, ſerves as a menſtruum for the food, and for diſpoſing it to be tranſmuted into chyle. But the firſt ſtep towards this event is taken in the craw. It is here that the aliment is penetrated by the œſophageal liquor, and begins to change its ſmell and taſte: that of the hardeſt texture is prepared to be broken down when it deſcends into the ſtomach, which in theſe birds may be ſaid to ſupply the place of teeth.

But the way in which the food deſcends from the mouth into the ſtomach, is deſerving of attention. When our fowls are abundantly ſupplied with meat, they ſoon fill their craw: but it does not immediately paſs hence into the gizzard, where it does not arrive till after it has been macerated in the craw: it always enters in very ſmall quantity, proportional to the progreſs of trituration in the ſtomach. Here then, what happens

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(*a*) Haller Elem. Phyſiol. T. 6. Valliſn. Op. in Fol. T. 1.

happens in a mill, may be obſerved to take place. A receiver is immoveably fixed above the two large ſtones which ſerve for grinding the corn; this receiver lets the corn which it contains, fall continually in ſmall quantity into the central hole in the upper ſtone, through which it paſſes, and diffuſes itſelf in the void ſpace between the two ſtones, where it is broken down, triturated, and pulverized by means of the ſtrong friction of the upper ſtone that moves round with great velocity upon that below. Meanwhile the flour paſſes from between the ſtones, as ſubſtances triturated by the gizzard, and diſſolved by the gaſtric juices, are expelled through the pylorus into the ſmall inteſtines.

LV. All this may be obſerved, by inſpecting the alimentary canal during the time of digeſtion. If the bird has fed upon grains, they are found in the cavity of the gizzard, partly entire, but ſoftened by a fluid. That part of the œſophagus that lies between the end of the crop and the beginning of the ſtomach, either contains no grains at all, or only a few quite entire. Trituration takes place in the gizzard only. Thoſe which have firſt entered this cavity, are found to have loſt the farinaceous ſubſtance, and are reduced to mere bran; the ſucceeding ones are more or leſs broken, and the laſt are entire. Amid this mixture of bran, and broken and entire grains, we always find a ſemifluid pultaceous maſs of a whitiſh yellow colour. This is the farinaceous part of the grains decompoſed by the gaſtric liquor, and converted into chyme. Meanwhile freſh grains continue to fall into the gizzard, in order to undergo the ſame tranſmutation: this admirable proceſs continues as long as the grains continue to fall into the ſtomach.

Theſe appearances and changes take place alſo in animal ſubſtances, whenever birds with muſcular ſtomachs feed upon them.

LVI. At whatever time the ſtomachs of theſe birds happen to be opened, they always contain a certain quantity of gaſtric liquor. This is leſs abundant when they are full of food, (for in this caſe it is abſorbed by the food) than when they have little or none. If we wiſh therefore to be provided with a large quantity of this liquor for experiments, it ſhould be taken from the empty ſtomach. Beſides, in this caſe it is purer than when mixed with the food. When examined in a ſtate of purity, its tranſparency, if we except a ſlight yellow tinge, is little inferior to that of water. It has likewiſe the fluidity, but not the inſipidity of water, being always a little bitter, as well as ſalt. I have found that the gizzards of turkies and geeſe moſt abounding in gaſtric juices, probably on account of their ſuperior ſize. I was induced by the quantity they afforded to attempt an experiment, which if it ſucceeded, would ſtill further prove that trituration is only an aſſiſting or prediſpoſing, and not the efficient cauſe of digeſtion. It conſiſted in trying, whether theſe juices retain their ſolvent power out of the ſtomach. For this purpoſe, I took two tubes ſealed hermetically at one end, and at the other with wax: into one I put ſeveral bits of mutton, and into the other ſeveral bruiſed grains of wheat, and then filled them with the gaſtric liquor. In order that they might have the condition which in theſe animals precedes digeſtion, they had been macerated in the craw of a turkey cock. And as the warmth of the ſtomach is probably another condition neceſſary to the ſolution of food, I contrived to ſupply it by communicating to the tubes a degree of heat nearly equal, by fixing them under my axillæ. In this ſituation I kept them at different intervals for three days, at the expiration of which time I opened them. The tube with the grains of wheat was firſt examined; moſt of them now conſiſted of the bare huſk, the flour having been extracted, and forming a thick grey ſediment at the bottom of the tube. The fleſh in the other tube was in great meaſure diſſolved, (it did not exhale the leaſt putrid ſmell) and was incorporated with the gaſtric juice, which was hence rendered more turbid and denſe. What little remained had loſt its natural redneſs, and was become exceedingly tender. Upon putting it into another tube, and adding freſh gaſtric liquor, and replacing it under the axilla, the remainder was diſſolved in the courſe of a day.

I repeated theſe experiments with other grains of wheat bruiſed and macerated in the ſame manner, and likewiſe upon ſome fleſh of the ſame kind, but inſtead of gaſtric juice I employed common water. After the two tubes had remained three days under my axillæ, I found that the grains, where they were broken, were ſlightly excavated, which was occaſioned by an incipient ſolution of the pulpy ſubſtance. The fleſh had alſo undergone a ſlight ſuperficial ſolution, but internally it appeared fibrous, red, firm, and in ſhort, had all the characters of fleſh. It was alſo putrid; the wheat too had acquired ſome acidity, two circumſtances which did not take place in the grains and fleſh immerſed in the gaſtric liquor. Theſe facts are then irrefragable proofs that the gaſtric juice retains even out of its natural ſituation the power of diſſolving animal and vegetable ſubſtances in a degree far ſuperior to water.

LVII. The gaſtric juice which I employed was taken from a turkey. That of a gooſe produced ſimilar effects. I have further found, that in order to obtain the ſolution of animal and vegetable ſubſtances, this juice ſhould be freſh. It loſes its efficacy, when it has been kept ſome time in veſſels, eſpecially if they ſhould happen to be open. It alſo becomes inefficacious after it has been uſed for an experiment. Laſtly, a conſiderable degree of heat, equal to the temperature of man or birds, muſt be applied; otherwiſe, the gaſtric juices are not more effectual in diſſolving fleſh and vegetables than common water. This artificial mode of digeſtion is well calculated to illuſtrate the ſubject I have undertaken to treat; but I ſhall have opportunities of ſpeaking of it at greater length in the ſubſequent diſſertations.

DISSERTATION II.

CONCERNING THE DIGESTION OF AMIMALS WITH AN INTERMEDIATE STOMACH. CROWS. HERONS.

LVIII. BY the term intermediate ſtomach, I mean ſuch a ſtomach as, on the one hand, is not properly muſcular, that is, provided with thick and ſtrong ſides, as in the gallinaceous family (I.); and on the other, is not merely membranous, that is, very thin, as in birds of prey and man, but has an intermediate degree of thickneſs and ſtrength. The ſtomachs of both the raven (*a*) and grey crow (*b*) may be conſidered in this light, though in reality they approach nearer to the muſcular than the membranous claſs. The intermediate power of theſe ſtomachs contributes alſo to characterize them; it is very

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(*a*) Theſe two ſpecies are called by Linnæus, Corvus cineraſcens, capite, jugulo, alis caudaque nigris.

Corvus ater, dorſo atro-cœruleſcente, cauda ſubrotunda.

(*b*) The hooded crow of Pennant. Corvus Cornix L.

very far from being equal to the force of muſcular, but greatly exceeds that of membranous ſtomachs. Such tubes of tin, as doves and pigeons would flatten and disfigure with the greateſt eaſe, remain unaltered in the ſtomach of crows. Thus alſo grain is triturated by the former, but continues whole in the latter. Their gaſtric muſcles however are not inert. They exert a certain degree of action, but it is far inferior to that of the gizzard in the gallinaceous claſs. Thus, though they cannot compreſs tin tubes, they are capable of producing this effect upon tubes of lead, provided they are very thin: and thoſe that continue unaltered at firſt, are at length ſlightly incurvated or diſtorted at the edges, and generally filled with fragments of the food, evident marks of conſiderable motion in the gaſtric muſcles; there are no effects which ſhew ſuch motion in animals with membranous ſtomachs, as we ſhall find in its proper place. I have often ſeen theſe phænomena, having kept a great number of grey crows and ravens: how uſeful they have been in the courſe of my enquiries the reader will learn from a peruſal of the preſent diſſertation.

LIX. Theſe birds, as well as man, may be denominated *omnivorous*. Herbs, graſs, leguminous ſeeds, fleſh of every kind, alive or dead, ſerve equally for their nouriſhment. As theſe two ſpecies poſſeſs powers for the concoction of various aliments, either entirely the ſame as, or ſtrongly reſembling thoſe of man, it is obvious, that the knowledge obtained from them will greatly illuſtrate the proceſs of digeſtion in us. They beſides ſeem formed on purpoſe to forward the views of the obſerver. When we wiſh to know what changes have been produced in ſubſtances incloſed in ſpheres and tubes, and given to gallinaceous birds, it is neceſſary to extract the tubes and ſpheres from their gizzards; that is, it is neceſſary to kill them. Hence for every experiment we muſt ſacrifice an individual, at no ſmall expence to our philoſophical curioſity. On the contrary, we can perform ſuch experiments upon crows as often as we pleaſe, without deſtroying a ſingle individual. With reſpect to ſubſtances, they are incapable of digeſting ſuch as the above-mentioned metallic receivers I have diſcovered, that they poſſeſs the privilege of returning them through the mouth, as birds of prey vomit the feathers and hair of the animals they have devoured, a circumſtance well known both to naturaliſts, and thoſe who train falcons for the field. But whereas this vomiting generally takes place every twenty-four hours in birds of prey, in crows it happens at leaſt every nine, and commonly every two or three hours.

LX. As I obtained the ſame reſults from both ſpecies, I will employ in my narration the generic name only (*a*). My obſervations were begun in winter, the moſt convenient ſeaſon for procuring a large number, owing to the multitudes, eſpecially of ravens, with

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(*a*) Corvus is the generic name in Latin, and Cornacchcia in Italian, and Crow may very well ſerve for it in Engliſh.

which Auſtrian Lombardy, and indeed almoſt all Italy then abounds. All the crows which I could obtain, when newly taken, had a large collection of pebbles in the ſtomach; the biggeſt were of the ſize of ſmall peaſe, the leaſt of that of millet: they were of various ſorts; I even found rounded pieces of brick. But in leſs than ten days not a ſtone remained in the body, a circumſtance which I learned from the inſpection of ſeveral ſtomachs, when I had occaſion to kill ſome crows in order to obſerve the anatomical ſtructure of the alimentary canal. They were voided partly at the anus, as it appeared from the excrements, and partly through the mouth; in the latter inſtances they were glued by the gaſtric liquor to the outſide of the tubes which I had forced them to ſwallow, and which they afterwards threw up. When unprovided with pebbles, they continued to eat and were nouriſhed as well as before. Hence it is to be inferred, that they are not more neceſſary to digeſtion in birds with intermediate, than in thoſe with muſcular ſtomachs, as we have ſeen above (XXXI). And as I inclined to believe, that the laſt-mentioned claſs do not pick up theſe ſtones from choice, but mere accident (XXXIII); ſo I conſider the matter likewiſe with reſpect to crows, having obſerved, that though unprovided, they never peck them eagerly, even when hungry, but ſwallow them only when they happen to be mixed on purpoſe or by chance with their food, and as it were concealed by it.

LXI. I began my experiments by putting entire ſeeds in the tubes (*a*). Theſe ſeeds were beans and wheat. The reader will eaſily perceive, that crows are not ſo ſtupid as to take the tubes ſpontaneouſly, but that it is neceſſary to force them down the throat, and to paſs the finger along with them till they are got into the ſtomach. This I executed in the way I had before done in animals with muſcular ſtomachs (III). The tubes were all thrown up in the ſpace of three hours. The beans and wheat appeared as at firſt, excepting that they were ſomewhat ſoftened and ſwelled by the gaſtric juice, which had penetrated ſome way into them. I replaced the grains in the tubes, and introduced them again into the ſtomach, where they remained two hours longer, without undergoing any further change. I repeated the ſame experiment a great number of times, and upon computing the ſpace during which the tubes had continued in the ſtomach, I found that it amounted to forty-eight hours; in this interval the ſeeds had ſuffered no other alteration, except being a little more moiſtened. The gaſtric fluid is therefore incapable of effecting the ſolution of theſe vegetable matters.

LXII. But we have before ſaid, that they were entire; on which account, this juice could not act upon the farcinaceous ſubſtance of the grain till it had traverſed the huſk; and this might have diminiſhed its efficacy. In order to determine how far the ſuſpicion was well-founded, it became neceſſary to re-

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(*a*) Theſe tubes were the ſame I uſed for gallinaceous fowls, and I continued to employ them in the ſequel.

peat the experiment upon the ſame ſeeds bruiſed. This was done, and four tubes full of the coarſe flour were given to a crow: they remained eight hours in the ſtomach, and proved the juſtneſs of my ſuſpicion; for upon examining the contents, I found above a fourth part wanting. This could ariſe from no other cauſe but ſolution in the gaſtric liquor, with which the remainder was fully impregnated. Another obſervation concurred in proving the ſame propoſition: the largeſt bits of wheat and bean were evidently much diminiſhed; this muſt have been owing to the gaſtric liquor having corroded and diſſolved good part of them, as the nitrous acid diluted with a large quantity of water, gradually conſumes calcareous ſubſtances. I replaced what remained of the ſeeds in the tubes, and committed them again to the ſtomach, wherein they remained, at different intervals, twenty-one hours; at the end of which period they were entirely diſſolved, nothing being left but ſome pieces of huſk and a few inconſiderable fragments of the ſeeds.

LXIII. Wheat and beans floating looſe in the cavity of the ſtomach, undergo the ſame alteration as in the tubes. When I fed my crows with theſe ſeeds, I obſerved, that before they ſwallowed them they ſet them under their feet, and reduced them to pieces by repeated ſtrokes of their long and heavy beaks. And now they digeſted them very well; nay, this proceſs was very rapid in compariſon of that which took place within the tubes. But when the birds either from exceſſive hunger or violence ſwallowed the ſeeds entire, the greateſt part of them paſſed out entire at the anus, or were vomited. We cannot therefore be ſurprized, that the gaſtric juice could not diſſolve them within the tubes, ſince it was incapable of effecting this proceſs within the cavity of the ſtomach, where its ſolvent power is far ſuperior.

LXIV. To avoid being tedious, I will not ſpeak of other ſeeds ſubmitted to the ſame experiments; ſuch as chicken-peaſe, French beans, peaſe, and nut-kernels. I will rather mention vegetable matters of a ſofter and more yielding texture, which did not require to be broken in order to be diſſolved; ſuch as crumb of bread and apples. Theſe ſubſtances are not only diſſolved within the tubes, but require a much ſhorter time than beans and wheat. Several bits of a ripe apple, weighing eighty-two grains, were put into ſome tubes, and diſſolved in the ſpace of twenty-four hours in the ſtomach of a crow. Four bits of another apple, weighing an hundred and three grains, were diſſolved in little more than fifteen hours. Of an hundred and ſeven grains of crumb of wheaten bread, there only remained eleven after the ſpace of thirteen hours.

LXV. From vegetable I proceeded to animal ſubſtances. The greedineſs which crows ſhew for theſe afforded a certain preſage, that they would be diſſolved within the tubes. I filled eight tubes with beef, and gave them to four crows, two to each. The fleſh was not minutely bruiſed, as in the caſe of gallinaceous fowls (XLII), but each tube contained an entire piece. An hour had ſcarce elapſed when one was thrown up. The fleſh, upon examination, did not appear to be ſenſibly diminiſhed, but it was thoroughly ſoaked in the gaſtric liquor. The juice was a little bitter, and of a yellowiſh green colour; the fleſh had acquired the ſame taſte and colour in ſeveral places. In an hour and three quarters two other tubes were vomited up; and now the fleſh began to ſhew marks of ſolution. The red colour was changed to a dark cineritious hue, and the whole ſurface was become flabby, and the coheſion of the parts was deſtroyed. In another tube, diſcharged after the ſpace of two hours and an half, the ſolution had made a greater progreſs. A dark covering of jelly ſurrounded the fleſh, which came away when it was touched with the fingers; when applied to the tongue, it hardly exhibited the flavour of fleſh. The ſolution had proceeded ſtill further in four hours. At the expiration of that time two other tubes were thrown up; in theſe the fleſh did not amount to half the original quantity. The remainder was ſurrounded by the ſame gelatinous covering, under which it preſerved its natural colour, fibrous ſtructure, and ſavour. There remained only two tubes, which were vomited up ſeven hours after they had been taken. Both were empty, the fleſh therefore had been completely diſſolved, except a few bits of jelly that adhered to the inſide. I never could perceive the ſmalleſt token of putrefaction either during the progreſs, or at the completion of the ſolution. And this obſervation, that I may not be under the neceſſity of repeating it continually, is to be extended to all the ſolutions performed not only by other crows, but by all the animals that I ſhall have occaſion to mention in this work; for I can aſſert with the utmoſt confidence, that I have never been ſenſible of the ſlighteſt ſtench either in fleſh or any other ſubſtance which I introduced in tubes or any other way.

Nothing could be more ſatisfactory than the information obtained from this experiment. It not only rigorouſly demonſtrates, that the gaſtric liquor of crows is the ſolvent of fleſh incloſed in tubes without borrowing the leaſt aid from trituration, but it throws ſtill ſtronger light upon the mode of operation of this menſtruum in the gallinaceous claſs. It begins by ſoftening the texture and altering the colour; next ſucceeds the decompoſition of the parts; this tranſmutes the fleſh into a kind of jelly of a taſte different from that of fleſh: the jelly is then more thoroughly penetrated by the juice and extracted out of the tubes, and in the ſtomach is changed into chyle. It appears alſo, that this fluid does not penetrate deeply into the fleſh, but acts on the ſurface only, diſſolving and removing one layer at a time, if we may ſo ſpeak, like other corroding menſtrua, until it comes to the innermoſt part, which it alſo ſoftens and melts.

LXVI. We have ſeen, that the fleſh in the tubes ſhewed no ſign of ſolution till an hour and three quarters had elapſed, and that this proceſs was completed at the end of ſeven hours (LXV). But are we to conclude, that this is the meaſure of the time required by the gaſtric liquor for this operation? or that it would have been accompliſhed in a ſhorter time, if the liquor had had free acceſs to the fleſh? for it is certain, that the tubes are no ſmall impediment to the gaſtric juice. What then would happen if the impediment was in part removed? and what when it is entirely taken away, by putting the fleſh looſe into the ſtomach? In order to ſolve the firſt of theſe intereſting queſtions, I enlarged the perforations in the ſides of the tubes as much as poſſible (VII), then filled them with beef, as before (LXV), and introduced them into the ſtomachs of ſeveral crows. I now perceived with pleaſure the ſuperior efficacy of the gaſtric liquor. In an hour and an half three of the tubes were thrown up, and above a fourth of the fleſh appeared to be waſted. Two other tubes were diſcharged in leſs than two hours, and contained little more than half of their original quantity. And before the completion of the fourth hour, the remaining tubes were entirely empty.

LXVII. Before I proceeded to the other queſtion, I thought of inverting the foregoing experiment (LXVI), and inſtead of allowing freer acceſs to the gaſtric juice, of impeding it more and more, and at laſt hindering it almoſt entirely. I began with employing the uſual tubes wrapped in cloth; this, although it was thin, was ſufficient to prevent the ſolution of the fleſh, which now did not begin to take place till three hours after the tubes were introduced into the ſtomach, and was not completed till ten had elapſed.

The linen in which the tubes had been wrapped was ſingle, I now doubled it in order to hinder the ingreſs of the liquor more effectually, and repeated the experiment in the ſame manner. The fleſh afforded no token of ſolution for four hours, and was not entirely diſſolved for a whole day.

Upon wrapping round another fold the ſolution did not begin till nine hours had elapſed, and at the end of a day the fleſh was ſcarce half conſumed. In other reſpects the gaſtric juice had acted upon the fleſh juſt as it does in open tubes, excepting only the ſlowneſs of its operation. It was become externally gelatinous, and incoherent in its parts. It was tinged yellow in ſeveral places; the taſte and ſmell at the ſurface were not different from thoſe of the gaſtric liquor.

I concluded theſe experiments, by trying what would be the effect of putting fleſh into tubes with only three or four holes. After they had continued nine hours in the ſtomach, the reſult was as follows: ſmall excavations of greater or leſs depth were made in the parts oppoſite to the pores, and from theſe excavations ſmall furrows wandered irregularly along the ſurface of the fleſh. The fleſhy fibres both in the cavities and furrows were become exceedingly tender, they had beſides loſt their red colour, and were turned yellow. The reſt of the fleſh was unaltered. From what has been ſaid before, the origin of the cavities and furrows evidently appears to have been derived from the gaſtric juice, which by inſinuating itſelf through the little perforations, had there diſſolved and deſtroyed the fleſh; the reſt remained entire, becauſe none of the juice could enter, if we except a very ſlender ſtream, which had produced the furrows.

LXVIII. Let us now proceed to the ſecond queſtion, by which we are led to examine how much more readily fleſh lying looſe in the ſtomach is digeſted than when it is encloſed in tubes. Taking ſome of the ſame kind of fleſh that had been uſed before, viz. beef, I parted it into two equal portions, one of which was again divided into ſmaller bits before it was put into the tubes, while the other was left entire. Each portion weighed eleven pennyweights. I next gave the tubes, which were eight in number, to a crow, and to another bird of the ſame ſpecies, equally healthy and robuſt, I gave at the ſame time the whole portion of fleſh, to which I had previouſly faſtened a thread. This thread, hanging out of the bird's mouth, and being wrapped round the neck, I could draw up and examine the fleſh at pleaſure. And that every circumſtance might be alike, I had taken care that the two crows ſhould have their ſtomachs empty. In thirty-ſix minutes one of the tubes was vomited, and at the ſame inſtant I drew up the fleſh from the ſtomach of the other crow. The latter was throughly imbibed with gaſtric juice, eſpecially the part that reſted upon the bottom of the ſtomach. It had loſt its redneſs, and was now of a dirty colour; it weighed forty-two grains leſs than at firſt; on the contrary, the fleſh encloſed in the tube retained its original weight.

The tube and the fleſh tied to the ſtring, were replaced in their reſpective ſituations; and in order that both might remain the ſame length of time in the ſtomach, I took care to return the tubes as they were thrown up. The fleſh was entirely diſſolved in three hours, when I immediately killed the crow that had the tubes. Upon collecting and weighing all the fleſh that remained in them, I found it to amount to about ſeven pennyweights. Hence in three hours and nine minutes it had loſt four pennyweights.

On the other hand, the fleſh tied to the ſtring was reduced to half a pennyweight, which conſiſted of a packet of membranous or cellular fibres, the fleſhy part having been entirely diſſolved. This experiment clearly ſhews, that fleſh left looſe in the ſtomach is more ſpeedily digeſted than when it is encloſed in tubes. And theory perfectly agrees with fact; for ſince ſolution is the effect of the gaſtric fluid, it is evident that the food, when looſe in the ſtomach, is attacked by a larger quantity than when defended by the tubes.

LXIX. Young crows, as well as all other young birds, eat more than the adult; hence I ſuſpected their digeſtion to be quicker. Having a neſt of the grey ſpecies brought me in June, I made, among others, the experiment related in the laſt paragraph. The reſult was very ſatisfactory. A quarter of an ounce of beef, faſtened as before, to a thread, had ſcarce touched the ſtomach, when the ſolution began, and in forty-three minutes was completed; but an equal quantity diſtributed in ſeveral tubes, required four hours and a half to be diſſolved. Upon opening the ſtomachs of the two young birds, I immediately perceived the cauſe of this rapid ſolution; they contained half a ſpoonful of gaſtric fluid; a quantity ſeldom met with in the ſtomach of adult crows. As the neſtlings require more food, Nature has furniſhed them with the means of an eaſier and more ſpeedy digeſtion.

It is ſcarce neceſſary to remark, that the experiments related in the LVth and following paragraphs, clearly evince this important truth, that the digeſtion of food is proportional to the quantity of gaſtric juice acting upon it. When this liquor comes in contact only with a few points, the decompoſition is very ſlow and inconſiderable (LXVII); when freer acceſs is allowed, the ſolution takes place more ſpeedily, and is more conſiderable, (LXV, LXVI); it is very rapid, when every obſtacle is removed, and the food is on all ſides expoſed to the action of the ſolvent liquor, (LXVIII, LXIX).

LXX. It is a queſtion of ancient date, and ſtill agitated by modern phyſiologiſts, whether certain carnivorous animals are capable of digeſting bone. Among the various points, which I propoſed to diſcuſs in the preſent work, I conceived that this well deſerved the reflection and attention of the philoſopher; I ſhall therefore both here, and in another part of my work, relate what I have obſerved on the ſubject. If we look at a crow and a bird of prey devouring an animal, we ſhall be diſpoſed to think that the latter has the power of diſſolving bone, but not the former. When, for inſtance, a hawk takes a pigeon, it firſt ſtrips the back, and devours the muſcular part of the breaſt; then proceeds to the entrails; and, laſtly, ſwallows the ribs, vertebræ and head, not even ſparing the feet and wings, if it ſhould happen to be very hungry. When the ſame bird is given to a crow, it ſets about ſtripping off the fleſh; but when it has picked this clean, it leaves the ſkeleton. This rejection of the bones, is however very far from being an indubitable proof, in the eſtimation of the philoſopher, that they are incapable of digeſting them. At moſt it inclines us to believe it probable; but ſuch probability requires to be confirmed by facts: and being engaged in enquiries of this nature, I could conveniently ſubmit it to the teſt of experiment. As I happened to be provided with ſome phalanges of the human toes, I encloſed two in one of the uſual tubes, which remained thirteen hours in the ſtomach. They weighed fifteen pennyweights at firſt; nor was this weight at all diminiſhed, or the bones in the leaſt ſoftened. In doubt whether the too great thickneſs of theſe bones might not have prevented the gaſtric juice from acting upon them, I had recourſe to ſmaller ones. Happening one day to find one of my crows dead in the apartment where I kept them, and the reſt aſſembled round the carcaſe in crouds, and devouring it eagerly, I took one of the tibiæ, broke it in two, and encloſed it in a tube. The tube continued a whole day in the ſtomach of another crow, but the bone was neither ſoftened, or diminiſhed in weight. The ſame thing was alſo obſervable, after the bone had been left looſe in the ſtomach for fourteen hours longer.

LXXI. The greedineſs with which the crows devoured their companion, induces me to digreſs, for the ſake of noticing a miſtake of the celebrated Dr. Cheyne. He pretends, that crows cannot digeſt the fleſh of their own ſpecies; and that when they happen to ſwallow it, they vomit it up again. "Ipſa Cornix (ſays Haller, on the authority of Cheyne) cornicis canem ingeſtam non poteſt coquere & deglutitam vomitu rejicit (*a*)." But the truth is, that the fleſh which my crows devoured agreed very well with them, nor did they throw any of it up again. Further, in order to determine certainly whether the above-mentioned writer had fallen into an error or not, I killed and plucked another crow, and threw it into the chamber where its companions were kept, when they immediately leaped upon it, and devoured it with the ſame avidity as they had done the other, without afterwards vomiting the leaſt particle of it. Upon killing and opening, three hours afterwards, one which appeared to have loaded its ſtomach more than any of the reſt, I found the fleſh partly diſſolved, and in the form of a ſemifluid pulp, and partly in the proceſs of ſolution, the very ſtate in which I had ſeen other fleſh.

LXXII. But let us return to the bones. It appears that, whether large or ſmall, they are alike inſoluble in the gaſtric juices of crows (LXX). But is this true likewiſe with reſpect to thoſe of a ſoft ſtructure, and which approach to the nature of a cartilage? This was preciſely what I wiſhed to know; and in order to determine it, I made uſe of another tibia, taken from an unfledged crow, and which therefore had not acquired its natural rigidity, though it was ſo hard as to break, when I tried to bend it: and now the gaſtric

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(*a*) Phiſiol. T. 6.

liquor was not inactive. Of fifteen grains, which it weighed at firſt, it had loſt five, when it had continued ſix hours in the ſtomach encloſed in a tube. It was become ſo ſoft, that it was capable of being bent into the ſhape of an arch. It continued to waſte and become ſofter; and when it had remained twenty-ſeven hours in the ſtomach, it was ſo much reduced as to reſemble a thin tube of paper. It was not at all gelatinous; and when it was preſſed between the fore-finger and thumb, it ſhewed ſome elaſticity, by recovering its former ſhape when the preſſure was removed. It was not ſcabrous either internally or externally, but had rather acquired a greater degree of ſmoothneſs during its ſolution. In five hours longer concoction it loſt the ſhape of a tube, and was totally reduced to pieces.

LXXIII. I tried other tender bones belonging to larger animals; and more or leſs of them was diſſolved, but with difficulty, and after a very long interval. The ſolution was more ſpeedy in young crows, probably on account of the greater abundance of their gaſtric juices (LXIX).

With reſpect then to the queſtion concerning bones, we muſt conclude that they are indigeſtible by crows, except only ſuch as, on account of their ſoftneſs, are rather to be conſidered as cartilage than bone.

LXXIV. In the preceding, as well as the preſent diſſertation, I have always ſpoken of the ſtomach as the place deſtined for the concoction of food. And in truth, whether we conſult antient or modern phyſiologiſts, or conſider my experiments, it will appear ſo clearly proved, that it would be abſurd to entertain a doubt of it. But it may be proper to enquire whether this operation belongs excluſively to the ſtomach in the birds in queſtion, or is partly carried on in the œſophagus. The foundation of this enquiry reſts upon the manifeſt decompoſition, which has been obſerved in that part of the food that is found in the œſophagus of ſome animals, as among others in the ſea-crow and the pike (*a*). In order therefore to aſcertain this point, I was led to make a few experiments, which I will relate after I have given a ſhort deſcription of the œſophagus and ſtomach in crows, and of the ſources of the reſpective liquors in theſe two cavities.

LXXV. The œſophagus is membranous, and has no craw. When dilated it is cylindrical, if we except a ſlight contraction in the middle. To the naked eye it would ſeem deſtitute of follicular glands, which however become conſpicuous when it is viewed with a glaſs. They are in ſuch abundance, that there is not a ſingle point of this canal without numbers of them. The excretory ducts are ſcarce diſcernible, though they emit the liquor of the follicles in great plenty. To ſee this, it is ſufficient to paſs the pulpy part of the finger over them. The liquor is of a viſcid nature, of a cineritious white colour, and ſomewhat ſweetiſh to the taſte.

The inferior part of the œſophagus, has the ſame kind of fleſhy faſcia that has been noticed in birds with muſcular ſtomachs. This

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(*a*) Helvetius Mem. de l'Acad. 1719. Plot Nat. Hiſt. of Staffordſhire.

faſcia in crows is ſcarce an inch long; and in them too, as well as in the claſs of birds juſt mentioned, is a tiſſue of large follicular glands very evident to the naked eye, of a roundiſh figure, and full of a ſweet fluid, leſs viſcid than that in the ſmall follicles in the membranous part of the œſophagus, but more denſe, and of a lighter cineritious hue.

LXXVI. In the gallinaceous tribe we have ſpoken of three coats, the cartilaginous, nervous, and muſcular (XLVIII, XLIX), which principally compoſe the ſtomach. Theſe three coats are likewiſe found in birds with an intermediate ſtomach. When the cartilaginous is ſeparated from the nervous coat, and the latter is viewed with the naked eye, it is ſeen to contain a multitude of whitiſh little bodies inchaſed in it, which have the appearance of points; but when examined by the microſcope, change their appearance to that of follicular glands, much ſmaller than thoſe in the fleſhy faſcia (LXXV); theſe follicles are full of a viſcid liquor, which they diſcharge at the extremity turned towards the ſtomach, when they are preſſed by the finger, or any other body. The diſcovery of theſe glands in the nervous coat having afforded me room for imagining, that they might empty their contents into the ſtomach, I examined the cartilaginous coat with great attention, in order to try if I could find any minute pores for the tranſmiſſion of the liquor into the cavity of that viſcus; but I acknowledge ingenuouſly, that I could not diſcover any. This however by no means proves their non-exiſtence; for they may be ſo ſmall as to evade the ſight, even when aided by the microſcope. And I cannot but believe, that theſe follicles, of which the excretory ducts are turned towards the ſtomach, are deſtined by nature for pouring their contents into that organ.

LXXVII. I now proceed to enquire whether, excluſively of the ſtomach, digeſtion is at all performed in the œſophagus of crows. In order to determine this, I firmly fixed to an iron wire two equal pieces of veal, one of them to the end of the wire, and the other two inches above. I then forced it down the throat of an hungry unfledged crow; the piece faſtened to the end lay in the ſtomach, while the other occupied the œſophagus. To prevent them being thrown up, a ſtring, faſtened to the upper end of the wire, came out at the mouth, and was tied round the beak. Thus I was enabled to draw up the fleſh at pleaſure, and examine how much it was diſſolved. In an hour the piece that lay in the ſtomach was quite conſumed, except a little cellular ſubſtance; but the other piece was entire. It was again introduced into the œſophagus, and re-examined an hour afterwards; but now the œſophageal liquor had begun to act upon the fleſh: its weight at firſt was ſix pennyweights, but now only five and an half. It was kept upon the whole ſix hours in the œſophagus, and loſt nearly two pennyweights. Theſe experiments will not permit me to refuſe the œſophagus all power of digeſtion, an effect undoubtedly produced by the fluid of the follicles (LXXV); but it is inconſiderable when compared with that of the ſtomach, ſince this viſcus diſſolved ſix pennyweights of fleſh in an hour, while the œſophagus diſſolved but two in ſix hours.

LXXVIII. The experiments I afterwards made on young crows were ſtill more deciſive. The ſame wire was employed, and two bits of fleſh were faſtened to it juſt in the ſame manner, the one occupying the ſtomach, the other the œſophagus. The former piece was generally quite diſſolved before the ſolution of the other began, though in time this alſo was very ſenſibly waſted. This diminution upon one occaſion amounted to five pennyweights in the ſpace of thirteen hours.

LXXIX. Laſtly, in order to determine whether it was part only of the œſophagus or the whole of that canal in crows which poſſeſſes the power of ſolution, I formed a cylinder of fleſh half an inch thick, and of the length of the œſophagus and ſtomach taken together. I faſtened this cylinder longitudinally to the wire employed before, and forced it down the throat of a crow, ſo that one end touched the bottom of the ſtomach, and the other reached almoſt to the mouth. In a quarter of an hour the whole circumference of the cylinder was imbibed with a fluid, but at the lower end only, which reſted upon the bottom of the ſtomach; the fleſh had begun to be diſſolved; here it was become whitiſh. In about an hour for near an inch, i. e. for the whole length of the ſtomach, ſcarce any of the cylinder was left; and what little remained was gelatinous and had loſt its coheſion, while the portion that lay in the œſophagus appeared to be unchanged: but it did not continue ſo; a ſort of eroſion began to take place along the cylinder, which went on, but with extreme ſlowneſs. And as this eroſion extended along the whole length of the cylinder, I had reaſon to believe the whole length of the canal capable of concocting the food in a ſmall degree, whenever it happened to be lodged there for ſeveral hours. But ſuch an event never happens when crows take their food at pleaſure, ſince the pieces never exceed the length of the ſtomach. In this reſpect they differ from ſome other animals, in which the food, after they have ſwallowed it, reaches into the œſophagus.

LXXX. Upon conſidering the great quantity of fluid continually dropping into the craw of gallinaceous fowls (LII), it appeared highly probable, that the concoction of the food is not a little promoted by the delay it makes there before it falls into the ſtomach. But the fact is juſt the reverſe. The aliment is indeed ſoftened and macerated (LII), but I could never perceive that it was at all diſſolved: at leaſt, I could never ſee any trace of it on ſeveral vegetable matters, which had been long retained in the craw. In a ſpace more or leſs ſhort they become ſoft, and are imbibed with a fluid; but I have not been able to perceive, that they were in the leaſt diſſolved. We muſt therefore conclude, that the œſophageal liquor in gallinaceous fowls is different from that of crows.

LXXXI. But why is the food ſo ſoon digeſted in the ſtomach, and ſo ſlowly in the œſophagus? Is it becauſe the gaſtric fluid is more efficacious, or in greater quantity, than the œſophageal? What are the properties and characteriſtic marks of theſe two liquors?

May we hope that experiments out of the body will be as inſtructive as thoſe made in it? To enable myſelf to procure a large quantity of theſe fluids at pleaſure, was the firſt ſtep towards the ſolution of theſe problems. And as ſuch a quantity could not be eaſily got by killing the birds, it became neceſſary to invent a contrivance for obtaining it from them alive. To put bits of dry ſpunge into the tubes, and leave them ſome time in the œſophagus and ſtomach, appeared to be the beſt means of attaining this end; for they muſt neceſſarily be ſaturated with the liquor of theſe cavities, and when vomited or drawn up, will ſupply the experimenter with a conſiderable quantity, provided he has uſed many tubes. Three tubes were introduced into the ſtomach of a crow, and four hours afterwards vomited up; the three little ſpunges, when they were taken out and preſſed between the fingers, afforded thirty-ſeven grains of gaſtric liquor, which was frothy, of a turbid yellow colour, had an intermediate taſte between bitter and ſalt, and being ſet to ſtand in a watch-glaſs, depoſited in a few hours a copious ſediment. As the ſediment appeared to ariſe from the food that was diſſolved by the gaſtric juice (for the bird had taken food a little before it ſwallowed the tubes) I repeated the experiment upon another crow, of which the ſtomach was empty, and continued ſo till the tubes were thrown up. This precaution I ever afterwards obſerved, at the ſame time taking care, that the faſt did not laſt too long, leſt it ſhould induce a morbid ſtate in the animal. I was likewiſe careful to cleanſe the ſpunges from every impurity, by repeated waſhings and dryings, before I made uſe of them again. Upon repeating, with theſe precautions, the foregoing experiment with the three tubes, I obtained thirty-three grains of gaſtric juice in a ſtate of purity. It differed from the former in being of a tranſparent yellow colour, and in depoſiting very little ſediment; it had the ſame bitter and ſalt taſte. It appeared to have very little volatility, as it was kept ſeveral days in a watch-glaſs without ſuffering almoſt any diminution. When thrown upon burning coals, it extinguiſhed them inſtead of taking fire; and when brought near a candle, it did not riſe in flame. Further, paper ſoaked in it and thrown upon the fire, did not burn till the gaſtric fluid was evaporated. It had not more volatility or inflammability when juſt taken from the ſtomach and ſtill warm.

LXXXII. The quantity, which was not inconſiderable, obtained from three ſpunges, gave me hopes of collecting enough for chemical experiments at large, and for attempting artificial digeſtion. Every crow was capable, as I found upon trial, of taking eight inſtead of three tubes; and as they would be thrown up in a few hours, I could repeat the experiment ſeveral times a day. Therefore to five crows, of which I then happened to be in poſſeſſion, I gave forty tubes furniſhed with little ſpunges, i. e. eight to each. In three hours and a half all the tubes were returned by the mouth, and the quantity of gaſtric fluid expreſſed from them amounted to four hundred and eighty-one grains. Many days did not elapſe before I had collected thirteen ounces of liquor. I employed it for the purpoſes for which I had deſigned it, and which ſhall be mentioned in their proper place.

LXXXIII. While I was engaged in theſe experiments, I obſerved ſeveral remarkable things. The firſt was, that the gaſtric juice flowed in great abundance into the cavity of the ſtomach; it ſometimes happened, that one of the ſpunges was brought up in a quarter of an hour after it had been ſwallowed, and in this ſhort ſpace it was conſiderably loaded, and in an hour as much as it could be. Secondly, when a conſiderable quantity of fluid has been obtained, another, and even a third may be got immediately. Sometimes when a crow has vomited up the eight tubes, I put freſh ſpunges into them and returned them without delay; and this I repeated a third time and found, that the quantity procured the laſt time was as great as the ſecond, and even the firſt. Thirdly, the fluid had always the qualities above-mentioned (LXXXI), if we except a difference of colour. It is commonly of a pale orange, but ſometimes of a cineritious yellow.

LXXXIV. I took the ſame method to procure the œſophageal liquor, with the variation only of a ſingle circumſtance. The tubes were now faſtened to threads, which were brought out at the mouth, and tied round the beak to prevent its being opened.

Thus the tubes were fixed in the œſophagus, without the leaſt danger of their getting into the ſtomach, or being thrown up. Beſides, I could draw them up at pleaſure. I introduced four tubes at once into the œſophagus of a crow, and extracted them in three hours. I learned from this firſt trial, the ſcantineſs of the œſophageal compared with the gaſtric fluid. The four ſpunges ſupplied me with eleven grains only. Doubting whether this might not be mere accident, I repeated the experiment ſeveral times, and allowed the tubes to remain longer in the œſophagus, but the ſpunges were very far from being ſo thoroughly ſaturated with fluid as in the ſtomach; ſo that direct experiment proves the great abundance of the gaſtric, in compariſon with the œſphageal liquor. If the ſtomach and œſophagus of a crow be laid open longitudinally, the latter will be found to be moiſtened with its proper fluid only, while the former generally affords reception to part of it likewiſe. Theory too, in the preſent caſe, agrees with fact. The natural poſture of crows, and indeed of moſt other birds, is ſuch, that the liquor which oozes out from the internal ſurface of the œſophagus, cannot but deſcend to the lower parts from the law of gravity, and thence into the ſtomach. This organ muſt therefore be the receptacle of the œſophageal fluid; but it is more than probable, that it has a peculiar fluid alſo (LXXVI): beſides, we are certain, that the bile is mixed in conſiderable quantity with the gaſtric juices. I have very frequently found the bottom of the ſtomach in crows full of it, and this is the reaſon why this juice is always bitter and yellow. Further, upon opening the duodenum longitudinally, I have perceived the yellowiſh green veſtiges of the bile, which is diſcharged into that inteſtine at the diſtance of at leaſt three inches from the pylorus through the cyſtic duct, which evidently ariſes from the gall-bladder. The conjunction of all theſe liquors, muſt produce a quantity of fluid far larger than that which derives its ſource from the œſophagus alone. And I doubt not but this is the reaſon, why the food is digeſted more ſpeedily and perfectly in the ſtomach than the œſophagus (LXXVII, LXXVIII). Though I ſhould alſo ſuppoſe, that this is in part owing to the greater energy of the gaſtric liquor from the admixture of the bile, which never riſes into the œſophagus, as it appears from the juice of that canal never being at all yellow or bitter, but nearly inſipid and colourleſs.

LXXXV. It remains now for me to ſpeak of ſome attempts to produce artificial digeſtion with the gaſtric juices, reſerving for another opportunity the recital of the chemical experiments made upon that fluid, obtained both from crows and other animals, with the view of acquiring as complete a knowledge as poſſible of its nature and properties. The greater abundance I was able to procure from crows, by means of vomiting, gave me the advantage of inſtituting a greater number of trials, than with that of gallinaceous fowls (LVI, LVII), from which the gaſtric liquor could not eaſily be procured without killing them. I firſt wiſhed to examine the effect of the gaſtric juices of crows upon fleſh, in the open air. It was January, and Reaumur's thermometer placed near the veſſel uſed for the experiment, ſtood at the fourth and fifth degree (*a*). For greater certainty in theſe experiments, I eſtabliſhed a term of compariſon, by employing ſimilar veſſels containing the ſame fleſh infuſed in water. I alſo took care upon the preſent as well as other occaſions, that the fleſh ſhould be completely immerſed in the reſpective liquors, and that the phials ſhould be cloſed with ſtopples. For ſeven days the fleſh kept in the gaſtric juice, and in water continued the ſame. On the eighth I perceived a ſlight ſolution, for upon agitating both liquors, ſeveral particles ſeparated from the larger maſs, and fell down to the bottom of the phials. No further progreſs was afterwards made, and the gaſtric fluid did not ſeem at all more efficacious than common water; only the fleſh immerſed in the former was preſerved from putrefaction, but not in the latter.

LXXXVI. In this experiment I had uſed beef; I verified the ſame obſervation upon the more tender fleſh of calves, chickens, and pigeons, notwithſtanding the heat of the atmoſphere had now raiſed the thermometer to ſeven degrees (*b*). While I was making theſe experiments in the natural temperature of the air, I was employed about others of a like nature in a warmer medium, viz. in a ſtove, in which the heat varied between (*c*) 22° and temperate. And now the effects pro-

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(*a*) Wherever the thermometer is mentioned in this work, the ſame, viz. Reaumur's is to be underſtood.

N. B. The fourth and fifth degrees of Reaumur's thermometer anſwer to about forty-two and forty-three and one-fourth of Fahrenheit's.

(*b*) Forty-eight and three-fourths of Fahrenheit's.

(*c*) Seventy-nine and a half of Fahrenheit's.

duced by the gaſtric fluid, differed from thoſe produced by water. In the latter the fleſh began to be a little diſſolved in two days; this was an effect of incipient putrefaction, as appeared plainly from a fœtid ſmell which began to be exhaled. The ſmell continued to increaſe during the following days, and in a week became intolerable, and the fleſh was reduced to a nauſeous pulp. In the gaſtric juice the ſolution was more rapid, and exhibited different phænomena; twenty-five hours were ſufficient to decompoſe the fleſh contained in it, and in a little more than two days there remained only a very ſmall morſel entire. Theſe ſolutions never emitted any bad ſmell; whence it is evident, that they did not ariſe from incipient putrefaction, like thoſe in water, but from a more efficacious and a different menſtruum, viz. the gaſtric liquor.

LXXXVII. Being now engaged by different occupations, I was obliged to interrupt theſe experiments, and could not reſume them till the June following; and then taking advantage of the ſeaſon, I expoſed to the ſun two phials filled to a certain height with gaſtric juice from crows, in one of which were immerſed ſeveral pieces of beef, and in the other crumb of wheaten bread. Nine hours of ſunſhine much forwarded the artificial digeſtion, which was the object of enquiry. A good part of the fleſh was reduced to a kind of glue, that when it was handled adhered to the fingers; nothing like fleſh remained in any of the pieces, but the nucleus, which was ſtill conſiſtent and fibrous, and theſe two qualities it loſt the next day; after having been expoſed ſix hours to the ſun the nuclei, like the outſide, no longer retaining a fibrous ſtructure. The heat in the ſun, as well on the firſt as the ſecond day, was between forty and forty-five (*a*). The gaſtric liquor produced upon the bread a change analogous to that which the fleſh had undergone. It not only loſt its white colour and was turned grey, but had become viſcous, and no longer preſented to the eye the appearance, though it retained ſomewhat of the taſte of bread. Of bread as well as fleſh immerſed in water and expoſed to the ſun for the ſame time, there was a perceptible diminution; but it was very ſuperficial and inconſiderable when compared with that produced by the gaſtric fluid. The bread turned ſour, and the fleſh became putrid, circumſtances that did not at all appear in the other phials.

LXXXVIII. Thus a tolerably complete concoction was obtained in the heat of the ſun; but it was reaſonable to ſuppoſe, that it would be ſtill more perfect in the temperature of the ſtomach. In the preceding diſſertation I have obſerved, that by way of ſubſtitute for the natural heat of the animal that furniſhed gaſtric liquor for the experiment, I fixed the tubes under my axilla (LVI, LVII); ſuch an expedient was neceſſary, ſince glaſs is incapable of reſiſting the violent impulſes of the gizzard. But there was now no longer the ſame danger, and the experiment might be made in the following manner. Several glaſs tubes ſix lines long and three in diameter, were hermetically ſealed at

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(*a*) An hundred twenty-two, and an hundred thirty-three and one-fourth of Fahrenheit's thermometer.

one end, and at the other bits of fleſh were introduced, and then the tube was filled with gaſtric fluid. It was then very carefully ſtopped with ſealing-wax, and the tubes were forced into the ſtomachs of ſeveral crows. Should digeſtion now take place it might be properly called artificial, ſince it muſt have been effected in cloſe tubes, to which the juices of the ſtomach could have no acceſs. But I ſoon found that the wax became ſoft in the animal heat, and conſequently did not keep the tubes cloſely ſtopt, as I wiſhed. There was however no difficulty in ſubſtituting a firmer cement, which would not be either melted or ſoftened; and with ſuch a cement I repeated the experiment juſt mentioned, and others of a like nature, which I will deſcribe hereafter. I prepared two tubes in this manner; they were given to a crow, and returned by vomiting in an hour and an half. I will not conceal my amazement at finding, that the pieces of fleſh encloſed in the tubes were not in the leaſt changed, unleſs it was in having acquired a blueiſh red colour. My amazement was ſtill more increaſed upon obſerving, that they had undergone no further alteration after remaining four hours longer in the ſtomach of the ſame crow, encloſed as before in two ſealed tubes. Theſe bits of fleſh weighed in all twenty-eight grains; ſo inconſiderable a quantity would have been diſſolved in a few minutes, if it had been looſe in the ſtomach, and in a very few hours, if it had been encloſed in tubes open at the ends.

LXXXIX. Did this unexpected failure ariſe from the communication between the external exernal air and that within the tubes being cut off, or from a deficiency of gaſtric fluid, or elſe for want of the action of the ſtomach upon the fleſh? I conſidered maturely theſe conjectural explanations, but they appeared altogether inſufficient. With reſpect to the laſt, it is repugnant to all thoſe facts which prove the ſolution of aliment within tubes, open indeed at the ends, and perforated along the ſides, but which effectually prevent the mechanical action of the ſtomach upon their contents. That the gaſtric fluid was in too ſmall quantity for the ſolution of the fleſh, is a ſuſpicion unworthy of attention; for the pieces were always covered by it, ſo that the quantity of fluid muſt have been greater than that of ſolid. Laſtly, the communication between the external air and that within the tubes being intercepted, cannot in all likelikood be the reaſon why ſolution did not take place. In order to determine this certainly, I made the following curious experiment. Having prepared ſeveral glaſs tubes of the length of ſix inches, I ſealed them hermetically at one end, by means of a reverberated flame, and the oppoſite extremities were drawn out ſo as to form elongated cones. Through the open end of theſe cones I poured a quantity of gaſtric fluid, together with a few ſmall pieces of fleſh, which filled two thirds of the wider part of the cone, I then introduced the cones by their baſis into the ſtomachs of ſome crows, allotting one to each bird; and when they reſted upon the bottom of the ſtomach, their apexes came out at the mouth. To prevent their being thrown up, I uſed the precautions mentioned in another place (LXXVI). Theſe conical tubes muſt no doubt have been very incommodious to the animals, but they were exceedingly well adapted to the end I had in view, ſince a free paſſage was allowed for the external air into them. However, notwithſtanding this, the fleſh remained ſeveral hours immerſed in the gaſtric fluid, without ſhewing any ſign of decompoſition.

XC. It is proper to apprize the reader, that when the ſealed tubes, or the cones, were kept long in the ſtomach, as, for inſtance, ten or twelve hours, the fleſh was generally reduced to a dark-coloured gelatinous pulp. But this did not remove my ſurprize at ſeeing ſo ſlow a ſolution in thoſe cloſe receivers, in compariſon with the rapidity of the proceſs in the ſtomach. The gaſtric juice was quite freſh, it was in ſufficient plenty, and the fleſh put in the tubes and cones was expoſed to the ſame degree of heat when it is in immediate contact with the ſides of the ſtomach.

If crows are killed during the proceſs of digeſtion, the bottom of the ſtomach generally abounds in gaſtric juice, which when compared with that expreſſed from the ſpunges, appears to differ a little, being more denſe and bitter, and of a yellow inclining to azure. The juice which is mixed with the food, and occupies the upper parts of the ſtomach, approaches more to the nature of that with which the ſpunges are imbibed. Having learned from experiment, that digeſtion proceeds moſt rapidly at the bottom of the ſtomach, on account probably of the gaſtric juice being more active and efficacious there from its immediate mixture with the bile, which gives it the yellowiſh azure hue and a bitterer taſte; I preferred this juice to that from the ſpunges, and repeated with it the experiments with the ſealed and conical tubes mentioned in the LXXXVIIIth and LXXXIXth paragraphs. But the event did not anſwer my expectation, no ſolution of the fleſh taking place till ſeveral hours had elapſed.

XCI. Upon comparing the laboratory deſtined by nature for the proceſs of digeſtion, and theſe receivers prepared by art to accompliſh the ſame end, I could diſcover but two differences; the fleſh in the veſſels undergoes the action of a fluid which is never renewed; while, on the contrary, in the natural laboratory it is continually ſubjected to the action of freſh juices, inceſſantly ſupplied by an innumerable multitude of follicular glands: Beſides, the gaſtric juices being confined within the cavity of the ſtomach, there is little or no evaporation; whereas, when expoſed to the air, and conſequently cooled, they cannot but loſe ſome of their more volatile and active particles by evaporation. Does then the ſlow ſolution of fleſh in cloſe tubes and in cones, depend upon the gaſtric juice being deprived by theſe two cauſes of part of that energy, on which digeſtion depends? I found from experiment, that the former cauſe, at leaſt, the want of renovation had great influence in retarding the ſolution. If, inſtead of perfectly cloſing the tubes, I left a ſmall perforation capable of allowing ingreſs and egreſs to the gaſtric juice, the ſolution of the fleſh took place much ſooner. The ſame thing happened, when, inſtead of leaving the ſame juice in the cones all the while, I was at the pains of changing it ſeveral times. But warmth is another condition abſolutely indiſpenſable for rendering the gaſtric fluid of theſe animals fit for digeſtion. When this liquor is kept in a temperature not more than four or five degrees above the freezing point, its ſolvent power is ſo much impaired, that it does not ſeem more efficacious than common water (LXXXV). This is alſo obſervable at ſeven degrees (*a*) (LXXXVI). In order to render the effects of the gaſtric juice perfectly ſenſible, a ſtronger heat is requiſite, as from ten to twenty-two degrees (LXXXVI). Still ſolution proceeds very ſlowly; to remedy this the animal heat is neceſſary, viz. about thirty degrees (*b*) (XC). And ſo remarkable is the effect of heat in this particular, that the very liquor, which, for want of being renewed, concocts fleſh ſlowly at thirty degrees (XC), effects this very ſpeedily at forty and forty-five degrees (*c*) (LXXXVII).

XCII. Every time I expreſſed the juice from the ſponges, I waſhed them in pure water, which was tinged yellow by the remains. After having made ſo many experiments on the gaſtric fluid in a ſtate of purity, I conceived it might not be altogether without its uſe, to make one with the water in

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(*a*) Forty-ſeven three-fourths of Fahrenheit's thermometer.

(*b*) Ninety-five one-half of the ſame.

(*c*) One hundred and twenty-two, and one hundred and thirty-three one-fourth of Fahrenheit's thermometer.

which the ſpunges had been waſhed; with this water I therefore filled a ſmall glaſs phial, which was left expoſed to the ſun, with a piece of fleſh in it, for three days in July. The fleſh (which was mutton) ſhewed ſome ſigns of ſolution. On the third day, there appeared upon the bottom of the phial a quantity of impalpable matter of a cineritious colour, conſiſting of particles ſeparated from the fleſh immerſed in the liquor. Notwithſtanding the ſeaſon, as it uſually is in July, was very hot, it had acquired little or no fœtid ſmell; whilſt a ſimilar piece of fleſh, expoſed to the ſun in the ſame manner, but immerſed in water, became intolerably putrid on the ſecond day.

XCIII. But it is time to quit the ſubject of digeſtion in crows, and to proceed to that of herons, the other ſpecies of bird which I propoſed to examine in this diſſertation. The herons upon which my obſervations were made, and which the nomenclators denominate *cineritious*, or *grey* (*a*), muſt certainly be claſſed among birds with intermediate ſtomachs, ſince the ſides of this viſcus have an intermediate thickneſs and ſolidity between membranous and muſcular ſtomachs. When this organ is dilated, it appears about two inches wide, and as many long; its form approaches to that of a cylinder. When opened lengthwiſe, and obſerved internally, it preſents the appearance of rugæ, of which ſome run in a longitudinal, ſome in a tranſverſe, and others in an irregular and oblique direction. The ſides of the ſtomach are co-

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(*a*) Linn. Syſt. Nat. T. 1. Bel. Av.

vered with a kind of gelatinous lining, of ſome conſiſtence, but eaſily removed, and of a colour between white and yellow. This lining ſeems organized; and I ſhould be inclined to ſuppoſe, that it is the innermoſt coat of the ſtomach. The nervous coat next preſents itſelf; it is of a whitiſh colour, and moderate thickneſs, but its texture is ſtrong, and it is not eaſily lacerated. When this coat has been cleaned and dried with a napkin, and then diſtended, and compreſſed underneath, it will be immediately covered with an effuſion of very ſmall and ſcarce viſible drops, which enlarging and approaching towards each other, form at laſt a thin aqueous veil. And if this veil be wiped away, and the nervous coat be again diſtended and ſqueezed, another like the firſt will appear; and in like manner a third, a fourth, &c. with this difference only, that the quantity of humidity is every time diminiſhed. There can be no doubt of this being a portion of the gaſtric fluid, diſcharged directly into the cavity of the ſtomach. I have employed the utmoſt attention in ſearching whether this liquor derives its origin from glands, or any analogous bodies, but could never diſcover either the one or the other; and therefore it muſt be ſuppoſed to be ſecreted by ſmall arteries, which open into the ſtomach, and depoſit their contents there. After the nervous, we have the muſcular coat, of a red colour, and ſcarce a line in thickneſs. It is compoſed by fleſhy ſtriæ, partly tranſverſe, partly longitudinal. The former appeared to me to occupy he ſurface only, the latter conſtitute the nternal ſtrata, and are continued to the termination of this coat. There is alſo another coat, conſiſting of cellular ſubſtance, and this is the laſt of all.

XCIV. The ſtomach always, and eſpecially when empty, contains more or leſs gaſtric fluid, of a bitter taſte, turbid yellow colour, and generally of ſome denſity. The bitterneſs is owing to the bile, which has this taſte, but in an intenſer degree; I have often found it at the bottom of the ſtomach, and in the vicinity of the pylorus. The gall-bladder in length exceeds an inch; it's greateſt diameter is of of five or ſix lines; in ſhape it reſembles a ſmall egg, of which the ſharp end is inſerted into the liver. Notwithſtanding many careful examinations, I am not certain of having found the cyſtic duct; I however ſuſpect that it perforates the duodenum, at the diſtance of ſix inches from the pylorus; this I collect from a line of an azure-yellow hue, which ariſes from the gall-bladder, and is inſerted into that part of the inteſtine.

XCV. Above the ſtomach we meet with the ſame kind of fleſhy faſcia, which I have noticed in gallinaceous fowls and crows, (LXVI, XLVII, LXXV). In the grey heron it exceeds an inch in breadth. This faſcia is alſo covered with the ſame gelatinous lining that inveſts the ſtomach (XCIII). Next we find the nervous coat of a finer texture than that of the ſtomach, of which it appears to be a continuation. This coat, when attentively viewed, looks like a ſieve, ſo much is it perforated at every part. The perforations are nothing but the apertures or mouths of ſubjacent follicular glands, occupying almoſt all the inſide of the faſcia, and viſible through it. If the nervous coat be any where compreſſed, a viſcid, cloudy, and, as far as I could judge, inſipid liquor oozes out at theſe mouths, and continues to ooze out if the preſſure be continued. The follicles that lie beneath, manifeſtly ſupply this fluid in ſuch abundance. It would be ſuperfluous to deſcribe theſe glandular bodies, ſince they exactly reſemble thoſe of crows and gallinaceous fowls; whether we conſider the immenſe number of them, or their poſition, contiguity, ſhape, or colour. When we raiſe this aggregation of follicles, we come to the muſcular coat, which is very thin, and conſiſts of ſeveral ſtrata of long and compact fleſhy faſciculi; next to which lies the laſt or external coat, the thinneſt of all, and conſiſting of cellular ſubſtance.

XCVI. The œſophagus is about twelve inches in length, and in breadth one and a half. Its ſhape is nearly cylindrical, but it becomes narrower near the ſtomach. Upon examining it externally with a microſcope, I diſcovered that it was quite full of minute bodies, which are, I ſuppoſe, glandular. When it has been carefully inverted and inflated, and the humidity, which always covers it, has been wiped away, if now we lay hold of one end, and ſqueeze it forcibly, ſo that it ſhall be enlarged in the adjacent parts, the humidity will appear again; and upon repeating the compreſſion, it will be ſeen ſeveral ſucceſſive times, juſt as in the ſtomach, (XCIII); with this difference, however, as I conceive, that the humidity in the ſtomach derives its ſource from ſmall arteries, and in the œſophagus from minute glands, or ſome analogous bodies.

XCVII. It was natural to ſuppoſe that this apparatus of fluids, which are conſtantly trickling into the cavity of the ſtomach and œſophagus of herons, are chiefly deſigned for digeſtion. But the ſmall number which I poſſeſſed, and their almoſt never vomiting, like crows, indigeſtible bodies, and conſequently tubes, did not permit me to make ſuch a ſeries of experiments as I could have wiſhed. I have however been able to make the moſt eſſential, of which one conſiſted in enquiring into the manner of digeſtion in the ſtomachs of theſe birds. For this purpoſe I had recourſe to the tubes, than which I do not believe there are any means better adapted to ſuch enquiries. It is well known that the grey heron feeds on fiſhes, frogs, waterſnakes, and ſeveral ſorts of aquatic worms and inſects. Thoſe in my poſſeſſion devoured frogs, and eſpecially fiſhes, with great greedineſs; and I therefore uſed them for my experiments. They ſwallow frogs of a moderate ſize whole. A whole frog, incloſed in a tin tube, was introduced into the ſtomach, together with a fiſh, of bulk nearly equal, included in another tube. In twenty-four hours the heron was killed, and the ſtomach opened; though the tubes were very thin, they had received no damage, if we except two ſlight contuſions upon one of them; they were ſo light, that it was not difficult to gueſs that they no longer contained the ſame quantity of matter that had been put into them. The little fiſh was all diſſolved, except ſome of the ribs, a few bones of the head, and a bit of the fleſh of the back, which was ſo tender, that the parts no longer cohered. The frog's ſhape was more eaſily diſtinguiſhable than that of the fiſh; the pulp of the thighs, and even the bone itſelf, was quite deſtroyed; but the ends of the lower, as well as the upper limbs remained. The integuments of the abdomen and thorax were no longer to be found; and the ſubjacent fleſh was become ſo ſoft, that it appeared to have undergone a ſlight boiling. The bones had acquired the ſoftneſs of cartilage. Theſe remains of the frog and fiſh were impregnated with gaſtric fluid, and taſted bitter. The intelligent reader perceives the immediate conſequences of this experiment. In the firſt place the ſtomach of herons acts with ſome force upon the ſubſtances it contains, as we may collect from the ſlight contuſions upon one of the tubes. Secondly, digeſtion, which was in an advanced ſtate in the frog, and quite complete in the fiſh, is not the effect of trituration, but of the gaſtric fluid, which entering in at the open extremity of the tube, or through the lateral holes, penetrated the two animals, and by virtue of its ſolvent power, partly conſumed them; but it made greater havock upon the fiſh than the frog, on account of its being tenderer. Thirdly, the efficacy of the gaſtric fluid of the heron is not limited to the ſolution of ſoft parts, ſuch as the ſkin, fleſh, &c. but extends to the hardeſt alſo, viz. the bones.

XCVIII. Of the laſt circumſtance I wiſhed to obtain greater certainty, by putting bones alone into the tubes. We have already ſeen that crows are incapable of digeſting hard bones, and that they digeſt ſuch as are tender with difficulty (LXX, LXXII, LXXIII). It was therefore an object of great curioſity to diſcover what would happen in herons; and it was eaſy to ſatisfy this curioſity, by incloſing bones of ſeveral ſorts in the tubes; in one I therefore encloſed only the tender bones of frogs or fiſhes; in another hard bones, viz. the thigh-bone of a turkey broken into two pieces. The pieces of both the hard and tender bones were formed into two bundles, and tied with thread. After an heron had retained theſe two tubes in its ſtomach twenty-ſeven hours, it was killed. It was with a mixture of ſurprize and pleaſure that I ſaw the tube which contained the fiſhes and frog's bones empty, while the ſtring remained entire. The gaſtric juice had then perfectly diſſolved the bones. But this was not the caſe with reſpect to the contents of the other tube. I ſhould have conſidered them as untouched, if they had not appeared ſmoother, whiter, and perhaps thinner than at firſt. They now weighed only eleven pennyweights and ſix grains, whereas at firſt they had weighed fourteen pennyweights; they had therefore loſt of their original weight three pennyweights within ſix grains. If this experiment be compared with that made upon crows, with the ſame intention, it will appear evident, that the gaſtric juice of the latter is leſs efficacious in diſſolving bones than that of the heron. And in truth, their nature requires that they ſhould digeſt every part of the animals upon which they feed. I gave them ſome frogs, and watched their way of eating them; when of a moderate ſize they ſwallow them whole, when very large they ſeparate them into ſeveral portions, and ſwallow them without parting the fleſh from the bones. Since, then, herons do not enjoy the advantage of vomiting up ſubſtances incapable of being digeſted (LXXVII), and the bones of frogs and ſuch animals cannot eaſily be voided at the anus, Nature has wiſely endowed the ſtomach with the power of concocting and aſſimilating bone.

XCIX. It was equally curious and important to enquire, whether the œſophagus of the heron as well as of crows, is capable of performing digeſtion (LXXVII, LXXVIII, LXXIX). The great length of the neck, and conſequently of the œſophagus, is extremely favourable to ſuch an enquiry. A flead frog was forced half way down this canal with the head downwards, where it was fixed by a ſtring, of which one end was tied round the hind legs, and the other came out of the mouth, and was wrapped round the neck. In this ſituation it remained two hours, and more was effected than I could expect in ſo ſhort a ſpace. The animal was indeed entire, but was become very tender, though the tenderneſs did not penetrate far below the ſurface. This appearance of incipient concoction induced me to puſh the experiment further, that I might ſee how it would terminate. The frog was therefore replaced in its former ſituation, where it continued three hours longer; I then thought it time to examine the animal, and drew up the packthread, but nothing came up with it except the hind legs and ribs; the reſt remained in the throat, and an inſtant afterwards I perceived the animal tranſmit it into the ſtomach. I found the legs and thighs half diſſolved, and being very deſirous of knowing what had happened to the other parts of the animal, I determined to kill the heron without delay. The frog was in the ſtomach; the external muſcular fleſh was quite deſtroyed, and what remained entire was eaſily diviſible into ſeveral portions, eſpecially at the articulations. The ſame appearance of decompoſition had taken place, as if it had been macerated in water; but it did not afford the ſmalleſt mark of putrefaction.

C. Although experiment thus abundantly evinced a ſenſible concoction in the œſophagus, I had not adverted to one circumſtance, which however deſerved attention, viz. to fix the preciſe loſs which the fleſh underwent in that cavity. I therefore repeated the experiment with this view, but having no frogs in my poſſeſſion, I ſubſtituted ſome fleſh with which I happened to be provided, conſiſting of cow's lungs, to the amount of half an ounce, forty grains. It was drawn out of the œſophagus by means of the packthread, after it had remained there thirteen hours. It had loſt ſeven pennyweights and two grains.

As the œſophagus of the heron is membranous, it is more than probable, that its mechanical action did not concur in producing this effect; it however was proper to prove this by direct experiment, which might be done by means of the tubes. With them therefore I repeated the experiment which I had before made, in order to determine, whether the œſophagus of the heron is capable of digeſting food. Solution of the fleſh undoubtedly took place within the tubes, and I was convinced, that it did not depend on any motion of the œſophagus, but on the efficacy alone of the fluid which is ſecreted by it.

CI. Another experiment yet remained to be made, whence we might deduce not only the exact diminution of the fleſh, but the proportion between its diminution in the œſophagus and ſtomach; two globular pieces, two-thirds of an ounce each, of cow's lights were introduced, one into the œſophagus, and the other the ſtomach; each remained ſeven hours in its reſpective ſituation, when the heron was killed; the ball from the ſtomach at firſt of the ſize of a walnut, was now no larger than a pea, and weighed only twenty-eight grains. That which had lain in the œſophagus was indeed reduced in bulk, though but very little in compariſon with the other; it had loſt three pennyweights eighteen grains.

Both theſe inſtances gave me an opportunity of remarking, that the juices, whether of the œſophagus or ſtomach, did not ſeem to act by penetrating deeply into the ſubſtance of the fleſh, but by corroding the ſurface; the external layer was firſt diſſolved, and then thoſe that lay beneath in their order. And in reality, when I came to waſh the ball taken from the œſophagus, and wipe away the external gelatinous ſtratum already diſſolved by the œſophageal fluid, the next ſtratum ſhewed the natural fibrous, firm, and red appearance; and when the ball was cut into two hemiſpheres, the inſide ſeemed perfectly ſound, without the ſmalleſt ſign of having been impregnated or touched by that corroſive liquor. The ſame obſervation is applicable to the other ball; for notwithſtanding its great diminution, within it was quite ſound.

I had now but two herons left, and theſe I ſacrificed to the deſire of aſcertaining ſtill further the exceſſively rapid concoction of the ſtomach, compared with that of the œſophagus. And in fact, I obſerved it again on two frogs and as many fiſhes, of which the former continued eight hours in the œſophagus and ſtomach, and the latter nine hours.

Theſe experiments incontrovertibly prove, that the œſophagus of the heron as well as of the crow, has the privilege of digeſting any food that may happen to be lodged in it: this privilege extends likewiſe to other animals, as we ſhall ſee in ſome paſſages of the following diſſertations.

CII. The obſervations related in the preſent and the preceding diſſertation, preſent us with various inſtances of agreement and diſagreement in the digeſtion of birds with muſcular and with intermediate ſtomachs. Let us here, for the convenience of the reader, collect into one point of view theſe ſcattered traits; they may fix more firmly in the mind all that we have obſerved, whether curious or intereſting, in theſe two claſſes of animals. With reſpect to the traits of reſemblance, they may all be reduced to the relation between the gaſtric fluids. Firſt then it has been proved, that theſe fluids, beſides being alike in colour, are always ſalt and bitter; and that the bitter taſte derives its origin from the bile, which regurgitates through the pylorus into the cavity of the ſtomach. Secondly, theſe fluids are the immediate agents of digeſtion, both in muſcular and intermediate ſtomachs, independently of trituration. Thirdly, In theſe two orders of birds the fluids act in the ſame manner in the ſolution of the food; they firſt ſoften and next convert the ſurface into jelly, then produce the ſame effect upon the interior parts, and ſo inſinuating themſelves gradually till it is completely diſſolved. Fourthly, they do not entirely loſe their ſolvent efficacy as ſoon as they are taken out of the ſtomach, provided they are heated to a proper degree, as artificial digeſtion proves. Laſtly, The fountains from which theſe fluids ſpring are, in great meaſure, the ſame in both claſſes, viz. the follicular glands, with which their organs abound.

CIII. The differences are in part reducible to the inferior efficacy of the gaſtric fluid in muſcular to that of the ſame fluid in intermediate ſtomachs. Thus the gaſtric fluid in the former is incapable of diſſolving the ſame aliment, which in the latter it eaſily diſſolves. In like manner the food, which each kind of gaſtric juice decompoſes and digeſts, is ſooner ſubject to this change from that which belongs to intermediate ſtomachs. And this is alſo the reaſon, why artificial digeſtion ſucceeds much ſooner with the firſt than the ſecond. The ſame inefficacy that the gaſtric juices of birds with muſcular ſtomachs ſhew in decompoſing certain aliments of a firm texture, extends alſo to their œſophageal juices in decompoſing ſoft ſubſtances, notwithſtanding the latter are tolerably well decompoſed by the œſophageal juice of birds with intermediate ſtomachs. The prodigious effects of trituration in muſcular ſtomachs, conſtitute another very ſtriking difference between theſe two claſſes of birds, the feeble force of intermediate ſtomachs being ſcarce comparable with the enormous power of the other kind. Such a degree of force was abſolutely neceſſary in theſe, ſince the juices are incapable of decompoſing food of conſiderable firmneſs, ſuch as ſeeds, the natural food of birds with gizzards; and therefore an agent capable of breaking, triturating, and thus pre-diſpoſing them for digeſtion became neceſſary; and ſuch are in reality the gaſtric muſcles in theſe fowls.

DISSERTATION III.

CONCERNING DIGESTION IN ANIMALS WITH MEMBRANOUS STOMACHS. FROGS. NEWTS. EARTH AND WATER-SNAKES. VIPERS. FISHES. SHEEP. THE OX. THE HORSE.

TO examine at full length the nature of digeſtion is the object of theſe diſſertations. By extending my enquiries to the three claſſes to which all animated beings may be referred, I hope to be enabled to ſolve the problem in a ſatisfactory manner. Of theſe claſſes the firſt comprehends animals with muſcular, the ſecond with intermediate, and the third with membranous ſtomachs. The laſt claſs is infinitely more numerous than the two former. If we ſuffer our imagination to range over the immenſe multitudes of quadrupeds, fiſh, reptiles, birds of prey, not even excluding man himſelf, we ſhall find, that they are all, or nearly all, endowed with membranous ſtomachs; not to mention that numberleſs tribe of minute beings, the greater part of inſects. My taſk would have been endleſs if I had projected enquiries, I will not ſay concerning every ſpecies of animals included under theſe genera, a project which many academies would not be able to execute, much leſs a ſingle obſerver; but concerning great part of them. I was therefore obliged to confine myſelf to reſearches upon a ſmall number. Theſe reſearches combined with others already related in the two firſt diſſertations, will be ſufficient, if I am not very much miſtaken, to ſet the theory of digeſtion in a clear point of view, both in animals and man. As the various ſpecies, which I take into conſideration, cannot be all exhauſted in a ſingle diſſertation, I ſhall diſtribute them into ſeveral, beginning with the animals that are ſituated loweſt in the ſcale of ſentient beings, and ending with that which occupies the higheſt and nobleſt place, with man.

CV. Let us begin then with frogs and water-newts, two ſpecies of ſmall carnivorous animals. As the mouth and œſophagus in the former are large, it was eaſy to introduce tubes into their long ſtomachs. But I was ſoon aware, that it would be neceſſary to make experiments upon a great number at once, if I wiſhed to know what changes the fleſh encloſed within the tubes underwent in the courſe of ſeveral days; for the tubes were very often vomited up, and at uncertain intervals, ſometimes in a few, ſometimes in ſeveral hours after they had been ſwallowed; at others again, after a whole day, and in ſome inſtances, after a ſtill longer interval. As I knew, that this ſpecies of animal very greedily devours any ſort of fleſh that falls in its way, I did not think of ſelecting, but took what happened to come firſt to my hands, and this proved to be a piece of the ſmall inteſtine of a ſheep, which I divided into twelve portions, and encloſed them in as many tubes. Theſe tubes were diſtributed among ſix of my largeſt frogs, two being allotted to each. They were kept in a very large veſſel of water with high perpendicular ſides, that they might not make their eſcape. I neglected the tubes that were thrown up, and examined thoſe only which remained in the ſtomach. In the ſpace of a day I obſerved the following reſults. From the intervals of the grating which lay before the open extremities of the tubes, there oozed out a cineritious matter, which, when touched, adhered to the fingers, and formed long filaments. When the grating was removed, I perceived, that this gluten was nothing but the fleſh itſelf, which at that part began to be decompoſed and to change its nature, retaining however the characteriſtic marks of fleſh in the more internal parts of the tubes. Upon opening the ſtomachs I did not find any gaſtric fluid; they were quite dry.

CVI. In two tubes that were examined at the expiration of two days, the fleſh had undergone a further decompoſition. It now not only oozed out at the meſhes of the lattice-work, but likewiſe at moſt of the perforations in the ſides of the tubes; and when it was drawn out with the point of a pair of forceps, and then freed by waſhing from the viſcid mucilage, what remained of real fleſh or inteſtine was ſo very little, that I do not believe it exceeded the thirtieth part of its original weight. At the end of the third day there remained but a ſingle tube in one frog; in this there was no fleſh, but it had been all diſſolved into gluten, had oozed out of the tube, and adhered to the ſides of the ſtomach, excepting a very ſmall portion that was ſticking to the tube. This viſcid matter was inſipid to the taſte, a certain proof that the gaſtric fluid had effected this alteration without the concurrence of any mechanical action of the ſtomach. It muſt however be allowed, that this fluid is exceedingly ſlow in producing its effects, ſince it required three days. This ſlowneſs muſt have ariſen either from the ſmall quantity or inefficacy of the fluid, or perhaps from both cauſes. In conſequence of this tardy action I found in ſimilar experiments upon ſix other frogs, that the fleſh in ſome of the tubes was not entirely conſumed at the end of the fifth day.

CVII. The gaſtric liquor of frogs is not however on this account incapable of concocting in time ſubſtances which we ſhould have ſuppoſed above its power, viz. bones. In a quantity of frogs brought me one day by the fiſhermen, there was one ſo large, that I was induced by its enormous ſize to kill it, in order to ſee what it contained; I found, that the enlarged bulk was owing to a mouſe in the cavity of the ſtomach. The hair had begun to fall off, and the ſkin was become ſo very tender, that it had loſt its coheſion. The fore as well as the hind legs had undergone a greater degree of ſolution, the naked bones only being left, and they were conſiderably waſted and converted into a ſemi-gelatinous ſubſtance. The mouſe upon being opened appeared quite ſound internally, the deſtruction was entirely confined to the ſurface, and therefore occaſioned by the gaſtric fluid, which had begun to act here on the external parts, juſt as it does in animals with muſcular and intermediate ſtomachs. The thinneſs of the extremities permitted the fluid to penetrate them with greater facility, hence it had almoſt conſumed them without ſparing even the bones. In this inſtance I could not perceive any ſign of trituration, for the mouſe was neither bruiſed nor lacerated; nor can I conceive what other force can be exerted by ſtomachs compoſed of ſuch fine coats, beſides that of compreſſing the bodies they contain, when they happen to be very large.

CVIII. The mouth and throat of water-newts are both ſo narrow, that they would not admit the uſual tubes, they however admitted others made in the ſame form, but of a ſmaller ſize, on purpoſe for them. From having kept theſe animals in my houſe for ſeveral years, both when I had occaſion to examine the circulation of the blood, and to obſerve the admirable reproduction of their limbs, I had learned, that the food which they devour with the greateſt avidity is living earth-worms (*a*). Nearly the ſame obſervation is made by my illuſtrious friend Mr. Bonnet,

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(*a*) I treat at full length of theſe aquatic lizards in my three works intituled,

Prodromo di un' opera da imprimerſi ſopra le riproduzioni animali;

Del Azione del Cuore ne' vaſi ſanguinei,

De' Fenomeni della Circulazione obſervata nel giro univerſale de' vaſi.

Bonnet, in his fine memoir *concerning the reproduction of the limbs in water-newts,* in which he confirms my diſcovery of this wonderful reproduction in the cleareſt and moſt deciſive manner, after it had been queſtioned by Meſſrs. Adanſon and Bomare, for want of addreſs and ſkill in making experiments on this branch of zoology (*a*). I had then recourſe to earth-worms; they were cut in pieces, and placed alive in the tubes, which were introduced into the ſtomachs of ſeveral ſalamanders. The gaſtric fluid of theſe little reptiles acted more ſpeedily than that of frogs (CVI). The divided worms began to change colour in fifteen hours, and to become ſoft and flabby. About the thirtieth hour the parts had loſt their coheſion, and the rings were no longer viſible; and in leſs than two days they were converted into a whitiſh pulp, of which the greateſt part had run out of the tubes.

CIX. The diſſection and examination of the ſtomachs of newts, preſented me with a phænomenon, which muſt not be concealed from the reader, both on account of its ſingularity, and the light it throws on the preſent ſubject. This phænomenon is nothing leſs than a great number of ſmall white worms in this viſcus, viſible to the naked eye; of the thickneſs of a thread, and the length (at leaſt the largeſt) of two-thirds of an inch; however if we wiſh to examine them minutely, we

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(*a*) The memoir is inſerted in Rozier's Journal for November, 1777.

N. B. It is likewiſe reprinted in the late collection of his works.

muſt employ the miſcroſcope. They are of two ſorts; in one both extremities terminate in a point, the other has one end pointed; but the other obtuſe, and marked with a dark ſpot; the latter ſpecies is ſhorter than the former, and thinner in the ſame proportion. Each ſpecies is furniſhed with rings, narrower at the ends of the body, and wider at the middle, as is generally the caſe in anular worms. Theſe two ſorts of worms are not flat or compreſſed, but round; it is therefore certain that they do not belong to the genus of *tænia*, or the *gourd-worm*, but to that of round or columnar worms (*teretes*). They are not looſe in the cavity of the ſtomach, as the worms lodged in the inteſtines of larger animals commonly are, but are always found with one extremity inſerted to ſome depth in the internal coat of that organ; hence it requires ſome force to detach them, and frequently they break ſooner than ſeparate. Of thoſe that have the dark ſpot, the more obtuſe end is fixed in the ſtomach; it is impoſſible to ſay whether this is the caſe with reſpect to others, ſince both ends are equally pointed. The looſe extremity projects into the cavity of the ſtomach, ſometimes coiled up in the form of a circle, and at others twiſted in a ſpiral. If the ſtomach be taken out of the animal, and ſet to macerate in water, the worms live for many hours without quitting their ſituation: if afterwards we ſeparate them with the hand, without breaking them, and place them upon ſome ſubſtance, in order to obſerve their movements, they will be ſeen to writhe in various directions; ſometimes bringing the mouth towards the tail; ſometimes ſtretching themſelves in a right line; and at others making ſtrange contortions, as is uſual with reptiles in general.

CX. Not being able to conjecture for what purpoſe the part perpetually inſerted in the ſubſtance of the viſcus, could be deſigned, unleſs it was to ſuck the thinneſt and pureſt part of the liquor; and conſequently ſuppoſing it to be the head of the animal, or at leaſt ſome analogous part, I tried to diſcover the mouth with the aid of the microſcope; but my endeavours were vain. I believe, however, that I found the alimentary canal; it is a bright ſilver-coloured ſpecies of inteſtine, running along the worm, in a tortuous manner, from ſide to ſide; it is always full of a number of particles, which fluctuate regularly, like a buoy, probably impelled by a ſort of periſtaltic and antiperiſtaltic motion. This canal is common to each ſpecies; in that with a dark ſpot at one extremity (CIX), a ſecond canal may be perceived; it is ſtrait, and probably (I ſhould rather ſay certainly) the receptacle for the eggs; for I have always obſerved it more or leſs full of a great number of corpuſcles, of an oval ſhape, floating in a very tranſparent lymph; theſe corpuſcles, when the worm is not in motion, always continue at reſt. If we lay hold of the animal by its extremities, and break it in the middle, the little canal will generally be broken, and the ovula will make their eſcape in a ſtream from the lacerated part. It is not difficult to burſt them between two pieces of talc, when a thin fluid ſpirts from them; after which the eggs become dry and opake, conſiſting now of nothing but the empty envelope, as always happens to the membranaceous eggs of ſmall animals. Each worm of this ſpecies is furniſhed with thoſe oval particles encloſed in their canal; if they are real eggs, as there is great reaſon for ſuppoſing, we muſt conlude that every individual is an hermaphrodite; it will however remain doubtful whether they are ſtrictly ſo; i. e. have no need of copulation, like ſweet water polypes, and many other ſorts of microſcopical animals, &c. or elſe, in the wider acceptation of the term, are like teſtaceous and naked ſnails and earthworms; each of which brings forth eggs and living young, but requires the concurrence of another individual.

CXI. It would not be an improper queſtion, if I were aſked whether theſe worms lodge in healthy newts, or rather in ſuch only as are diſeaſed. This doubt ſuggeſted itſelf to me; and in order to clear it up, I examined not only ſuch as I kept at home, and were therefore liable to the ſuſpicion of unhealthineſs, but ſuch alſo as were newly caught, and full of health and vigour; but the ſtomachs both of the one and the other harboured alike theſe unpleaſant gueſts. But it muſt be obſerved that they do not fix their abode in all newts; and that in thoſe where they do, they are not equally numerous. Of the immenſe number I have opened at different times, and with different views, three-fourths have had a family of worms in their ſtomachs; which is ſometimes compoſed of only five or ſix individuals; at others of ſeveral dozens, and at others again of an hundred or more.

CXII. In my numerous examinations of the ſtomachs of the different animals mentioned in this work, crows alone have exhibited a phænomenon nearly reſembling what is found in newts; I mean a quantity of worms lodged in the ſtomach. But theſe worms are not inſerted into the internal coat, as in newts, but are found between the internal and the nervous. We are very well acquainted with the little worms that live in trees, and generally fix their abode between the bark and the wood; and lurking there unſeen, devour the cortical part, which furniſhes them with an agreeable aliment. If the bark ſhould be parted from the trunk on purpoſe, or by accident, their devaſtations are expoſed to view, in the form of excavated paths, winding backwards and forwards in a ſerpentine direction; nor is it uncommon to ſurprize the worms actually employed in forming theſe excavations, which ſerve them at once for food and lodging. The ſame thing nearly is obſervable with reſpect to the worms of crows. If the internal be parted from the nervous coat ſlowly and carefully, theſe animals are ſuddenly expoſed to the eye, adhering for the moſt part to the back of the internal coat, lurking in certain cavities formed in its ſubſtance, and which in all likelihood, ariſe from the eroſion of theſe very worms. Further, we find ſome with both ends expoſed, while the middle is deeply buried in the ſubſtance of the internal coat. Laſtly, others have one extremity inſerted into this, and the other into the adjacent nervous coat; but they never make their way into the cavity of the ſtomach. Theſe worms do not appear to differ from thoſe in newts, in colour, length, thickneſs, or in the alimentary canal; they have however one eſſential difference, they are without rings, but have a ſmooth and ſlippery ſkin. In their motions they are dull and languid; when taken from their abode, and placed in water, they live many hours. They are found both in grey crows and rooks; but I have never ſeen them in any part of the body except the ſtomach.

CXIII. But let us return to the worms in newts (CIX, CX, CXI), and conſider them in as far as they relate to digeſtion. I aſſert that their preſence is an incontrovertible proof, that no ſenſible degree of force is exerted by the ſtomach; for how is it poſſible to conceive that the ſides of the ſtomach can rub againſt each other, or at leaſt impinge againſt the food, without doing the ſmalleſt injury to the delicate ſtructure of theſe worms? I have more than once taken the ſtomach of a newt between my thumb and finger, and compreſſed it very gently, or rubbed it lightly, and upon opening it, have always found ſome rupture, ſome diſcontinuation in the parts of theſe worms. We muſt therefore conclude, that digeſtion in water-newts is ſolely the effect of the gaſtric fluid, of which the efficacy has been already ſhewn in the decompoſition of earth-worms incloſed in tubes (CVIII). I have alſo ſeen this efficacy, in a manner equally ſtriking, on worms which newts have taken and ſwallowed of their own accord. How tenacious theſe minute reptiles are of life, is abundantly proved by cutting them into ſeveral pieces, in conſequence of which they do not die; but on the contrary multiply, as many worms being produced as parts into which they were divided (*a*). It is true, they do not ceaſe to live after having remained ten or twelve hours in the ſtomach of a newt; nay, when they fill it too full, they void ſome alive and crawling by the mouth, whether by actual vomiting, or whether the worms, after various movements in ſo diſagreeable a place of confinement, at laſt find their way out through the œſophagus. But they certainly die at laſt, not becauſe they are triturated or cruſhed to pieces, for they continue whole ſeveral hours; the gaſtric fluid firſt ſoftens and then converts them into a gelatinous ſubſtance, and by a continuance of its action, at length reduces them to an impalpable maſs.

CXIV. But how came the tender worms in the ſtomach to eſcape ſolution, when all other inſects, whether aquatic or terreſtrial, upon which the newt feeds, die and are digeſted? If it ſhould be ſaid, that this happens becauſe they have been habituated to the ſtomach by long reſidence, the difficulty would be perhaps removed to a greater diſtance, but certainly not taken away altogether. As the cauſe of this phænomenon, we muſt aſſign the inability of the gaſtric fluid to decompoſe theſe minute beings, however powerful may be its energy upon others of a ſtructure leſs delicate; juſt as a chemical

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(*a*) See Reaumur, Bonnet, and my Proſpectus.

menſtruum is capable of diſſolving one metal, but not another. Thus aqua regia diſſolves gold, but not ſilver; or an acid that combines with the calcareous, has no attraction for the argillaceous and ſiliceous earths. Nearly the ſame difference of digeſtion is alſo obſerved in that of polypes provided with arms; they ſometimes ſwallow their own arms along with inſects; but though the former die and are digeſted, the ſecond do not in the leaſt ſuffer. Thus a polype inſerted into the ſtomach of another polype, continues to live as before (*a*).

CXV. But let us proceed to ſerpents, of which I propoſed to treat after frogs and newts. Thoſe which are moſt eaſily procured in the environs of Pavia are certain terreſtrial ſnakes, called in ſome provinces of Italy, *Smiroldi* (*b*); water-ſnakes, which many naturaliſts call ſwimming (natrices) (*c*). The firſt conſiderably exceed the natrices and vipers in ſize. The largeſt are about an inch and a half in thickneſs towards the middle of the body, and forty-five and ſometimes fifty inches long. The lower part of the body is white mixed with yellow and green ſtreaks, the upper part is blackiſh, but towards the neck and head interſperſed with a milky white. They fly with greater ſpeed than the water-ſnakes, and far greater than

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(*a*) Trembley, Mem. ſur les polypes.

(*b*) Not deſcribed by Linnæus or any other naturaliſt, as far as I know.

(*c*) *Natrix*. Linn. Syſt. Nat. T. 1. Natrix torquata. Ray. quadr.

vipers. They are not inferior to the latter in a ſpirit of revenge, and their bite alſo draws blood, as I have myſelf experienced, but is harmleſs. Before I made uſe of the tubes, I wiſhed to acquire ſome knowledge of the œſophagus and ſtomach. Having therefore ſkinned one, and blown up the œſophagus in ſuch a manner that the air could neither paſs out above nor at the pylorus, it appeared to me to reſemble a large inteſtine, cylindrical for about the length of nine inches, and becoming gradually narrower below, ſo as to form a funnel of the length of four inches and an half. I ſoon perceived, that this funnel was the true ſtomach, and the inteſtine the œſophagus. Both the trachea and lungs run along the œſophagus, to which they are firmly attached by means of a membrane, as alſo is the heart, which has the ſhape of an elongated pyramid, ſituated at the origin of the lungs. We find likewiſe a viſcus ariſing from the baſis of the heart, aſcending upwards along the œſophagus, and adhering in great meaſure to the trachea: it is of the ſame length as the lungs, but its ſubſtance is different, being tender and aſh-coloured; I could not then determine what it was. Next below the lungs lies the liver, which, together with the *vena portarum,* reſembles a long narrow leaf attached to a very long footſtalk; both adhere to the œſophagus. Below the ſtomach we find the ſpleen, nine lines in length and of a very acute oval ſhape. The gall-bladder lies in the region of the ſmall inteſtines, conſequently at a great diſtance from the liver; when we preſs it the duct is filled with bile, which it evidently diſcharges into the duodenum at about the diſtance of an inch from the pylorus. Near the gall-bladder we find another body ſmaller than it, attached to the duodenum, and of a fleſhy conſiſtence. I ſhould ſuppoſe it to be the pancreas.

CXVI. If we ſeparate the œſophagus and ſtomach from the lungs and other parts juſt deſcribed, and open it longitudinally, the œſophagus appears ſimply membranous; the membrane of which it conſiſts is very thin and of a ſilver colour. The ſtomach is compoſed of thicker ſides, and among the coats which compoſe it we have one of fleſh, which like the fleſhy coats of other membranous ſtomachs, is very thin. I could not perceive, that the œſophagus is provided with any glands or follicles; but I obſerved, that the ſtomach was abundantly ſupplied with them throughout its whole length; they diſcharge part of their liquor on being preſſed, and the internal coat is moiſtened with it.

CXVII. I come now to experiments relative to digeſtion. I found great facility, not only in paſſing the tubes into the ſtomach, but likewiſe in bringing them up whenever I pleaſed. I made an aſſiſtant lay faſt hold of the ſnake ſo as to prevent its ſtriking or wreathing round the body, while I opened the mouth and forced a tube in lengthwiſe, and then, by means of a thin rod, thruſt it two or three inches down the throat. After this the reſt followed of courſe; for I had only to preſs with my fore-finger and thumb the neck of the animal in the place oppoſite to the top of the tube, which was forced to deſcend for ſome way down the œſophagus;

and by a repetition of the ſame manœuvre I ſoon brought the tube to the bottom of the ſtomach, which I knew by the reſiſtance it made when I attempted to puſh it lower; for now the narrow paſſage of the pylorus prevented its deſcent. By a like preſſure, but made in the oppoſite direction, from below upwards, I could bring up the tube from the ſtomach into the œſophagus, and thence out through the mouth. I employed this contrivance for introducing the tubes into the ſtomach, and bringing them out at the mouth in water-ſnakes likewiſe, and even vipers, managing the laſt however with ſome care, which is very requiſite, in order to avoid being bit by theſe ſerpents during the operation, when they are highly exaſperated.

CXVIII. When I was opening ſome of my land-ſnakes (*ſmiroldi*) to examine the alimentary canal, I found in the ſtomach of one a wall-lizard not in the leaſt injured or digeſted. I thought of employing it for my experiments, as it muſt be a kind of food well adapted to theſe reptiles. I therefore encloſed a piece of the tail of this lizard in a tube, which continued for a whole day in the ſtomach without its contents being at all diſſolved. Thirty-ſix hours produced ſomething more. The tail of the lizard is compoſed of a number of little muſcles, enchaſed one within the other, and bound round by a thin anular membrane. The piece of tail was placed in the tube in ſuch a manner, that the inveſting membrane was in contact with the ſides, and the muſcles were bare at the open ends. The membrane had ſuſtained no injury, but the muſcles were eroded on the plane of ſection, and a little a little excavated. Upon touching them I found, that they had been converted into a gluten of ſome viſcidity. The gaſtric fluid then (for the mechanical action of the ſtomach could produce no effect within the tubes, were any ſuch action to be exerted at all) had begun to digeſt the fleſh, by diſſolving what lay at the ends of the tube before it attacked that which was contiguous to the ſides; not only becauſe it was not covered by the membrane, but alſo becauſe it had freer acceſs at the ends: the ſolution however went on, though very ſlowly; for after the tube had been five days in the ſtomach, a little of the muſcular fleſh remained, and the membrane was almoſt entire.

CXIX. The fleſh of a lizard's tail is rather tough, and it was probable, that this circumſtance had retarded the progreſs of digeſtion; it was therefore proper to employ ſome of a leſs firm texture; accordingly part of the liver of the ſame animal was encloſed in the tube, and given to a ſnake (*ſmiroldo*). In this inſtance digeſtion was more ſpeedy; for in three days and an half the tube was quite empty.

But what if inſtead of encloſing the fleſh in tubes, we ſhould introduce it into the ſtomach without any covering? It was obvious to ſuppoſe, that it would be ſooner digeſted, ſince the gaſtric juice would have freer ſcope for its action. And ſo in reality it happened. A piece of lizard's tail of the ſame ſize as in a preceding experiment (CXVIII), did not require quite two days for its digeſtion; and a portion of liver, equal to that before-mentioned (CXVIII), underwent the ſame proceſs in thirty-two hours. Of this I aſſured myſelf by opening the ſtomachs of the two ſnakes, one of which had taken part of the liver, and the other of the tail.

CXX. We come now to the water-ſnakes or the *natrices*. Nothing can be more ſtriking than the reſemblance between the ſtomach and the œſophagus in this, and the foregoing ſpecies. Beſides the trachea, lungs, heart, liver, vena portarum, having nearly the ſame configuration, and lying on the ſame parts of the œſophagus; this cavity is very capacious and long, conſiſts in like manner of thin membranous coats, and ends in a funnel, which is the true ſtomach of the animal. The gall-bladder too is about an inch diſtant from the lungs, and depoſits its contents in the duodenum, by means of the cyſtic duct. The ſtomach alſo, as we have obſerved in the land-ſnake (*ſmiroldo*), is furniſhed with a great number of follicular glands.

CXXI. It is eaſy to learn the nature of the food of water-ſnakes, and we ought in conſequence to provide it for our experiments. Among the antients Oliger Jacobeus, where he treats of frogs, and among the moderns Valiſneri will ſatisfy us, that theſe reptiles live chiefly upon frogs. Next to man water-ſnakes may be denominated their greateſt ſcourge. They particularly frequent the water of ditches, puddles, ponds, lakes, ſuch in ſhort as is frequented by frogs; and here they make an eaſy prey of them, notwithſtanding they mutually give each other notice when they perceive the ſnake at a diſtance, by a kind of whiſtle or outcry of diſtreſs,

treſs, as I have often obſerved, at which all fly with the utmoſt precipitation: Dante was not unacquainted with this circumſtance.

> Come le rane innanzi l'inimica
> Biſcia per l'acqua ſi dileguan tutte,
> Finchè alla terra ciaſcuna ſ'abbica (*a*).

A fiſherman having brought me three very large and vigorous water-ſnakes, I gave each at the ſame time a tube encloſing a different part of a frog; one had muſcle, the other liver, and the third ſpleen. The tubes were left three days and an half in the ſtomach. Upon forcing them out, I obſerved the ſame kind of concoction that I had before ſeen in frogs (CV, CVI). The fleſh was beginning to be changed into an adheſive cineritious gluten; the interior parts were unaltered. The tubes were now introduced a ſecond time into the ſtomachs, and when they had continued there two days they were found empty; ſome of the adheſive matter ſtuck to the outſides of two of them.

CXXII. It is not unknown to naturaliſts, that this ſpecies of ſnake has no teeth, and is conſequently obliged to ſwallow its prey whole. In ſummer I have often taken them with whole frogs in the ſtomach. It was therefore not unreaſonable to ſuppoſe, that they are capable of digeſting the bones; and the leſs ſo, as it ſeems difficult for them to be voided backwards, on account of the narrowneſs of the inteſtines. It might indeed be ſuſpected, that theſe bones are vomited, as I have found to be the caſe with

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(*a*) Infern. Cant. 9. Fol. 161.

the tubes, both in this and the former ſpecies; but this is not a conſtant and regular evacuation, as in crows (LIX) and birds of prey, as we ſhall ſee hereafter; but takes place at uncertain intervals, and ſometimes does not happen at all for ſeveral days. In order however to aſcertain the fact, I broke two tibiæ nine grains each to pieces, encloſed them in the tubes, and forced them into the ſtomachs of two water-ſnakes. After they had continued there two days, they were become ſoft, and had loſt three grains. In five days more they were ſtill ſofter, and now weighed together only five grains. Soon afterwards the two ſnakes died, and it was not in my power, though I wiſhed very much to proſecute this curious experiment as far as it would go. From the beginning however of the progreſs we may ſuppoſe, that the bones would have been totally diſſolved, and conſequently it is highly probable, that water-ſnakes digeſt the bones of thoſe animals upon which they feed.

CXXIII. By the activity of the gaſtric fluid of the water-ſnake in digeſting not only fleſh but bone, I was induced to try to procure a quantity that I might examine it more particularly. For this purpoſe I employed ſpunges as before (LXXXI, LXXXII), and my ſucceſs exceeded my expectations. Six little ſpunges, that had lain two hours in the ſtomachs of three ſnakes, encloſed in tubes, afforded me enough to fill a watch-glaſs of a moderate ſize. It had the following qualities; the colour approached that of ſoot, it had the fluidity of water, and evaporated very ſlowly: it has both a ſalt and bitter taſte, and is not inflammable. Hence it appears to bear a very ſtrong reſemblance to the gaſtric fluid of the other animals, upon which my experiments have been made: this reſemblance extends likewiſe to the odour, which is exactly like that of the ſame juices in birds of prey, of which I ſhall ſpeak particularly in the next Diſſertation. I reſerve the account of ſome chemical experiments made upon this fluid, till I ſhall have an opportunity of ſpeaking of the examination of the other gaſtric juices which I have already mentioned, or ſhall have occaſion to mention in the preſent work.

CXXIV. We have before ſeen the ſtrong analogy between the configuration of the ſtomach and œſophagus in land and water-ſnakes. In vipers theſe cavities have the ſame general form; nor do they differ with reſpect to the efficient cauſe of digeſtion. I repeated upon them moſt of the experiments deſcribed above: ſeveral tubes, furniſhed with different ſorts of fleſh, were left in their ſtomachs for a ſpace more or leſs long, and the effect was juſt the ſame as in water and land-ſnakes; it would therefore be ſuperfluous to deſcribe them particularly. It will be better to turn the reader's attention to ſome experiments on theſe three ſpecies of reptiles differently modified, but relative to the ſame ſubject.

CXXV. Having frequently opened theſe animals when newly taken, I have ſometimes obſerved, that their ſtomachs are not large enough to contain the whole prey, and that part lies in the œſophagus. This part never ſhewed any mark of concoction, notwithſtanding what lay in the cavity of the ſtomach was ſometimes half digeſted. Thus, for inſtance, I have found five or ſix large beetles in the body of a land-ſnake or viper; thoſe that lay in the ſtomach were ſcarce diſtinguiſhable, while, on the contrary, thoſe in the œſophagus were entire, or nearly ſo. I once ſaw a frog with the lower limbs, which projected out of the ſtomach, not at all damaged, while the reſt of the body lay in the ſtomach and was half reduced to a pulp. Theſe experiments made by the ſerpents themſelves gave me reaſon to ſuppoſe, that what takes place in them is exactly contrary to what happens in crows and herons, for the reader will remember, that in theſe birds the œſophagus is really capable of digeſtion (LXXVII, LXXVIII, LXXIX, XCIX, C); but in the animals in queſtion it ſeems to belong excluſively to the ſtomach. A very ſimple experiment was ſufficient to aſcertain the point. Into the ſtomach of one of theſe ſerpents a frog, for inſtance, might be ſo introduced that part ſhould lie in the œſophagus. The frog might be faſtened to a cylinder of wood, and thus firmly fixed in the ſame place. The cylinder ſhould touch the bottom of the ſtomach with its lower extremity, and reach ſome way above that organ. I applied this apparatus to a water-ſnake, and at the end of the ſixth day opened it longitudinally. Upon examination my inclination to believe that the œſophagus was without efficacy, was changed into firm perſuaſion. The lower limbs, the part of the animal that had lain in the ſtomach, had nothing left but the bare bones, whereas the whole body which had extended into the œſophagus had ſuffered no injury.

CXXVI. The experiments related in the CXVIIth and following paragraphs were made in April, when the animals had lately quitted their ſubterraneous lurking places, and ſtill retained ſomewhat of that torpor which benumbs them during winter. At this time digeſtion, as we have ſeen, is a very ſlow proceſs. Are we to preſume, that when they become more lively, active, and vigourous, as the heat of the ſeaſon increaſes, they likewiſe perform digeſtion more ſpeedily? for the effect of heat in promoting the operation of the gaſtric fluid appears from other facts (LXXXVII). This idea was ſuggeſted by reperuſing the fine memoirs of the illuſtrious Trembley on polypes, in which the influence of the temperature of the atmoſphere upon the digeſtion of theſe wonderful animals is evident; inſomuch, that the very food which in a hot ſeaſon is completely digeſted in twelve hours, when it is cold requires ſometimes two or three days. In order to determine whether the ſame thing takes place in my reptiles, I choſe July for a term of compariſon, when the difference, if any exiſted, muſt needs be more ſtriking, as the thermometer in the ſhade ſtood at 22° and 23° *; whereas in April, when the firſt experiments were made, it did not riſe above the twelfth or fourteenth deg. †. And now upon repeating the experiments already deſcribed, I found that heat has ſome power in accelerating digeſtion, but not ſo much as I had

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* Eighty-one and an half; and eighty-three and three-fourths, F.

† Fifty-nine, and ſixty-three and an half, F.

ſuppoſed. Fleſh in the tubes did not require above two days to be completely digeſted; and when an equal quantity was introduced into the ſtomach by itſelf, about half that time was ſufficient.

CXXVII. Naturaliſts were already apprized of the tardineſs of digeſtion in ſerpents. In Bomare we read an account of a ſerpent at Martinico, which retained a chicken in its ſtomach for three months, and did not completely digeſt it, for it ſtill preſerved ſome traces of its ſhape, and the feathers ſtill adhered to the ſkin (*a*). It is a circumſtance deſerving of particular notice, and which I ſhall have occaſion to apply in another place, that fleſh does not become fœtid from remaining long in the ſtomachs of theſe cold animals, as I have obſerved in the courſe of my experiments, and eſpecially in a viper, which having been kept above two months in my houſe, could not but be unhealthy; this individual retained in its ſtomach for ſixteen days a lizard, which had been previouſly macerated in the gaſtric fluid; nor could I perceive that it had any odour, except that of this juice. And yet ſuch was the heat of the ſeaſon, that another lizard, about the ſame ſize, which I had placed out of curioſity in a cloſe veſſel, containing a little water, emitted an inſupportable ſtench before the expiration of the third day.

CXXVIII. But what can be the cauſe of this ſlowneſs of digeſtion in ſerpents? As they are cold animals, that is to ſay, as their blood very little exceeds the temperature of the

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(*a*) Dict. d'Hiſt. Nat.

the air, it may ſeem probable that this phænomenon might be owing to the want of that heat, which is peculiar to animals of warm blood. And I ſhould not have been unwilling to admit this cauſe, if other animals, with blood equally cold, had not enjoyed the privilege of digeſting their food in a much ſhorter period, as we ſhall ſoon ſee (CXXXIV). We cannot aſſign a deficiency of gaſtric fluid as the reaſon, for their ſtomachs abound with it (CXXIII). I cannot attribute it to any thing but the inefficacy of the fluid itſelf; and this is by no means ſingular, for we have already diſcovered a circumſtance nearly ſimilar in animals with muſcular ſtomachs, in which the gaſtric juices do not ſo ſoon digeſt the food, as in animals with intermediate ſtomachs (CIII).

CXXIX. Among fiſhes I will firſt treat of that ſpecies which bears ſo ſtrong a reſemblance to ſerpents, and is even conſidered in the chain of animated beings, as the intermediate link between fiſhes and ſerpents, I mean the eel. The ſtomach in this animal varies from the ſtructure generally obſerved by nature; it is not a canal immediately continued with the duodenum, but a kind of blind gut, of conſiderable length, ending in a point; after the food has been received into this gut, and been digeſted, it muſt aſcend, and return to the upper part of the ſtomach, in order to paſs into the duodenum, which forms an acute angle with that upper part. The natural figure of both may be ſeen in Blaſius's Anatomy of Animals (*a*).

(*a*) Plate LII. Fig. 1.

Into the ſtomachs of four eels I introduced ſeveral tubes, containing pieces of fiſh, the food moſt agreeable to eels. In order to preſerve them alive, I turned them in a ſmall ſtew, whence I could take them at pleaſure. They were killed at the end of three days and eighteen hours; and the tubes were found at the bottom of the ſtomach, entirely covered with a dark-coloured mucus, which, on attentive examination, appeared to be the remains of the fiſh, that by this time was digeſted. Upon wiping the tubes, and examining the inſide, five out of eight were empty, and the three others contained a bit of fleſh of the ſize of a vetch, but it had loſt its coheſion.

CXXX. This experiment abundantly proves, that in this fiſh digeſtion is produced by the gaſtric fluid; I therefore proceeded to experiments upon ſuch as are more juſtly entitled to the appellation of *fiſhes*. I choſe for this purpoſe carp, barbels, and pikes, as they were the moſt eaſy to be procured. It has been long well known that the alimentary canal in many ſcaly fiſhes, is provided with one or more blind appendixes, which, becauſe they lie in the vicinity of the pylorus, have been named *pylorici*; they are always full of a white, mucilaginous, and ſaltiſh fluid, which is diſcharged into the canal, and derives its origin from a number of glands lying in the appendixes. In ſome ſpecies they are few, in others in conſiderable numbers, and in others again exceedingly numerous; they amount in the ſturgeon to an hundred; in thoſe ſpecies, in which they are moſt numerous, the ſeveral faſciculi meet in a common duct; hence, notwithſtanding

ſtanding their numbers, they diſcharge their contents into the pylorus (*a*) by a few mouths. In the three ſpecies I have juſt mentioned, this ſingular apparatus is not to be found; but the inſide of the ſtomach and inteſtines is furniſhed with yellow bodies, that probably contribute in ſome way or other to digeſtion, though I have not been able to aſcertain their preciſe uſe. At firſt ſight they look like anular worms, adhering, as in newts, to the internal ſurface of the ſtomach (CIX); but if we lay hold of them with the forceps, the anular form vaniſhes, and we find that they are real appendiculi to the ſtomach and inteſtines. When ſtretched out, they are three lines long; each adheres to the villous coat by a footſtalk. If we ſtretch them till they break, a conſiderable quantity of yellow liquor iſſues out, and the body becomes ſhrivelled; and if it be now removed from the place of its inſertion, we find under it a little tumour, through which a globule ſhines obſcurely; and if the tumour be cautiouſly raiſed, appears diſtinctly: it is of a yellowiſh white colour, from the liquor which it contains. Are theſe globules cluſters of glands, and the vermiform bodies elongated ducts for conveying the liquor into the cavity of the ſtomach? I would very willingly have adopted this notion, if I had not found that theſe ſubſtances, when compreſſed from the lower end upwards, never diſcharge their contents, either from the ſummit, or any other part, contrary to what happens when we ſqueeze the folli-

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(*a*) Haller, El. Phyſ. T. 6.

cular glands in birds with muſcular, intermediate, or membranous ſtomachs. On this account I ſuſpend my opinion on the ſubject; although I ſhould incline to ſuppoſe that they are of ſome uſe in digeſtion.

CXXXI. Immediately under the teeth, the very beginning of the œſophagus in the carp, is moiſtened with a conſiderable quantity of a whitiſh turbid liquor, of a viſcid conſiſtence, and inſipid taſte, which when wiped off is inſtantly reproduced: and we here find a number of white papillæ, broad at their baſis, and terminating in a point, which, when preſſed, emit the ſame kind of fluid. If we make a gentle preſſure any where near theſe papillæ, a fluid iſſues out, but, as I ſhould ſuppoſe, of a different nature, ſince it is tranſparent, thinner than the former, and not at all viſcid. With the œſophagus, which is very ſhort, and of conſiderable thickneſs, is continued the ſtomach, of a membranous ſtructure, and very thin. It is eaſy to diſtinguiſh two coats in this organ, the internal and the nervous; in the latter thoſe globules, which left me in doubt whether I ought to conſider them as cluſters of glands, are buried (CXXX). In this ſhort deſcription, we ſee ſources capable of ſupplying the ſtomach with a large quantity of fluid, notwithſtanding it wants the *pylorical* appendixes.

CXXXII. The conformation of the ſtomach in the barbel, does not correſpond either with that of the carp, or various other fiſhes. The œſophagus, ſtomach, and inteſtines conſtitute a ſingle gut, nearly as in earth-worms, and a variety of inſects; this gut is only a little dilated at the ſtomach, and contracted at the commencement of the inteſtines. I could not diſcover within this canal any veſtige of glands or analogous bodies. However, both the œſophagus and ſtomach are continually moiſtened with a fluid in great abundance; which when we preſs or dilate either of theſe cavities, is ſeen to tranſude from the internal ſurface; and ſince, according to every appearance, it does not ariſe from glands, we muſt ſuppoſe that it comes from the open extremities of ſmall arteries terminating here.

CXXXIII. The ſtomach of the pike has the ſhape of a bag or ſack, of much greater length than breadth; it is full of longitudinal rugæ, of a light fleſh colour, and compoſed of coats ſo thin, as to be ſemi-tranſparent. The rugæ extend upwards into the œſophagus, which is eaſily diſtinguiſhed from the ſtomach by its white colour, and greater thickneſs. There is no appearance of glands either in the one or the other, though both, and eſpecially the ſtomach, abound with liquor.

CXXXIV. As fiſhes are ſubject to vomiting, the tubes which I had introduced into the ſtomachs of my carps, barbels, and pikes, were frequently returned; and I was often chagrined at finding them, after a few hours continuance in the body, at the bottom of the veſſel uſed for keeping them alive. However, from frequently repeating my experiments, though ſo many tubes were ejected prematurely, a few remained ſeveral hours in the ſtomach; and theſe were ſufficient to ſatisfy my wiſhes. In the preſent caſe, the ſame thing happened which I had ſo often obſerved in other animals; the fleſh was digeſted within the tubes, and that in a much ſhorter ſpace than in ſerpents (CXXVI, CXXVII). This obſervation was verified on the barbels, carp, and pike; the two latter ſpecies exhibited a phænomenon, too cloſely connected with the preſent ſubject to be omitted. Happening one day to open a pike, I found within it a little fiſh, about three inches in length, lying longitudinally along the ſtomach, ſo that the whole head occupied the œſophagus. I had here a clear view of the origin and progreſs of digeſtion. The jaws of the ſmall fiſh retained their natural colour, and appeared unaltered. The eye was beginning to quit the orbit, and the gills had loſt their purple hue, and were become very ſoft. In the ſtomach the marks of digeſtion were more evident. The fleſh of the body was more and more tender as I proceeded downwards; and towards the bottom it had degenerated into a ſoft and ſhapeleſs maſs. The extremity of the tail, which had lain at the bottom of the ſtomach, was entirely conſumed, and with it the vertebræ of the ſpine, and the adjacent bones.

CXXXV. I met with another ſimilar circumſtance in a little carp. It had ſwallowed a ſmall lamprey, which was ſtretched out at full length, and occupied the whole ſtomach, and at leaſt two thirds of the œſophagus. The part that lay at the bottom of the ſtomach was changed into a kind of mucilage, in which there was no appearance of any organized part, except ſome of the dorſal vertebræ. The parts that lay higher ſtill cohered, but they came away from the animal on being touched. The others, which occupied the œſophagus, ſhewed likewiſe marks of an incipient concoction.

Nothing can be more inſtructive than theſe two facts combined. They ſhew, in the firſt place, that the bottom of the ſtomach digeſts more quickly than the parts ſituated above, as we have ſeen in other animals (XC): ſecondly, that the œſophagus, as well as the ſtomach, is in ſome meaſure capable of concoction, a circumſtance that has been already noticed with reſpect to crows and herons (LXXVII, XCIX, C, CI); and which phyſiologiſts have obſerved before me in other fiſhes. Laſtly, that digeſtion in the œſophagus is ſlower at its beginning, and in its progreſs; two things that have been remarked in the birds juſt mentioned.

With reſpect to the triturating power of the ſtomach in theſe three ſpecies of fiſhes, not to mention that digeſtion has been obtained in the tubes without its concurrence, I am of opinion that it has no exiſtence; this I infer, from no effect being produced by it upon the tubes, upon which I have never perceived the ſmalleſt bruiſe, contuſion, or injury, in my experiments on fiſhes, any more than on frogs, newts, and ſerpents, though they were ſo thin, that the ſlighteſt force would have been more than ſufficient to diſtort or bruiſe them.

CXXXVI. From cold animals let us proceed to ſome experiments on the ſtomachs of warm animals, ſuch as ſheep, oxen, and horſes. Reaumur, in his ſecond and laſt Memoir (*a*) concerning digeſtion, after relating at length

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(*a*) Hiſt. de l'Acad. Roy. An. 1752.

his obſervations on a kite, ſlightly touches on ſome experiments upon dogs and ſheep. I will here quote the reſults of his experiments on the latter ſpecies, reſerving thoſe on the two others for another place. Deſirous of ſeeing whether digeſtion in ſheep is the effect of the gaſtric fluid, he forced down the throat of one of theſe animals four tin tubes, two of which were full of freſh blades of graſs, and the two others of chopped hay. Fourteen hours afterwards the ſheep was killed and opened, when the four tubes were found in the firſt ſtomach, with their contents; and the graſs and hay were not in the ſmalleſt degree digeſted, and but little macerated.

Suſpecting that they would undergo further alteration, and be even digeſted by a longer continuance in the ſtomach, Reaumur cauſed eight other tubes to be prepared in the ſame manner, i. e. four to be filled with freſh graſs, and the remaining four with hay. The graſs before it was put into two of theſe tubes, and the hay before it was put into two others, were moiſtened with human ſaliva. All the eight were forced down the throat of a ſheep, which was killed thirty hours afterwards; during this interval, the animal had been kept ſtrictly faſting; this precaution had alſo been obſerved with reſpect to the former ſheep, that had not retained the tubes ſo long. In the courſe of the thirty hours, the greater part of the tubes were voided at the anus, but a few remained in the firſt ſtomach.

But neither had the graſs or hay undergone the ſmalleſt degree of digeſtion; they preſerved their original form and dimenſions; and when they were pulled at the two oppoſite extremities, reſiſted efforts to break them with the ſame force that ſimilar pieces of graſs or hay, that had been a little macerated, would have done. Hence it is inferred by this illuſtrious naturaliſt, that digeſtion cannot be effected in the ſtomachs of ſheep by a ſolvent, unleſs that ſolvent be aided by trituration: he was however ingenuous enough to confeſs, that theſe two experiments are of themſelves very far from throwing ſuch light upon the preſent ſubject as he could have wiſhed.

CXXXVII. The firſt thing I undertook with reſpect to ſheep, was to repeat exactly Reaumur's experiments. Thinking the tubes I had hitherto employed too ſmall, I had ſome made eight lines in length, and four in diameter. But I could not at firſt introduce them into the ſtomach. After I had put them into the throat with my hand, though I puſhed them as far as my fingers would reach, they were always returned. With Reaumur's method I was unacquainted, for he does not give the leaſt information about it. At laſt an expedient occurred; it conſiſted in putting the tubes, provided with their contents, into a hollow cane, and introducing this cane into the œſophagus; I could now puſh them forwards with a rod, till they dropped out at the lower end of the cane into the œſophagus; and as the part of the œſophagus into which the tubes were now introduced, lay at a great diſtance from the mouth, the animal, in ſpite of all the efforts he made to return them, was obliged to receive them into the ſtomach: to the ſame contrivance I had recourſe likewiſe in oxen and horſes. Six tubes were given to a ſheep; in twenty-ſeven hours it was killed and opened; it had eaten nothing during all the time it retained the tubes; and this precaution was ſtrictly obſerved upon every ſheep upon which experiments were made. Notwithſtanding ſo long a faſt, the firſt ſtomach contained a large quantity of graſs, ſomewhat triturated; and though it had fed upon this before the experiment, it was not yet digeſted. In the midſt of this graſs, that was thoroughly imbibed with a greeniſh fluid, with which great part of the ſtomach was filled, lay five of the tubes; the ſixth had paſſed to the ſecond ſtomach, which may be conſidered as an appendix to the firſt. The herbs which I encloſed in the tubes, after they had been impregnated with my ſaliva, were beet, trefoil and lettuce; in three tubes they were green, and in the reſt dry. Upon opening all the ſix, I could not perceive that either the freſh or the dried plants had ſuffered any diminution, or undergone any degree of real concoction; it was only become a little tenderer, and the freſh herbs had loſt their green colour; in ſhort, the reſult of this experiment was exactly like that of Reaumur's.

CXXXVIII. I ſhould then have ſuppoſed, that in theſe animals digeſtion depends on the triturating power of the ſtomach, if it had not ſtruck me, that, as the herbs encloſed in the tubes had not paſſed further than the firſt ſtomach, they might not perhaps have felt the influence of that kind of gaſtric fluid, which is requiſite for the concoction of the food: for it is very poſſible that this fluid may reſide in ſome of the other ſtomachs, and eſpecially in the fourth, in which the aliments of animals with four ſtomachs, ſuch as ſheep, are always found in the ſtate of a very ſoft paſte. Reaumur indeed did not perceive any ſign of digeſtion even in the tubes that had been voided at the anus, and conſequently muſt have paſſed through the other ſtomachs. But this obſervation reſted upon a ſingle experiment; and to illuſtrate ſtill further a matter of ſuch importance, it could not but be proper to repeat it. I therefore treated another ſheep in the ſame manner, and allowed it to live thirty-ſeven hours afterwards, that the tubes might have time to paſs beyond the firſt ſtomachs. They did in fact paſs beyond them; and I found all ſix in the fourth, which anſwered the end I had in view; the three ſpecies of plants however, mentioned above (CXXXVII), both the green and the dry, were entire, and ſeemed only a little ſoftened by maceration.

CXXXIX. I was now about to declare in favour of the neceſſity of trituration in this animal, when a doubt occurred. Neither Reaumur nor myſelf had adverted to a circumſtance, which ever precedes digeſtion, both in ſheep, and every other quadruped endowed with four ſtomachs, as goats, oxen, deer, &c. I mean rumination. We are taught, both by diſſection and daily experience, that the food, when it has arrived at the ſecond ſtomach, does not immediately proceed to the third, and thence to the fourth, but, on the contrary, returns, and re-aſcends up the œſophagus; and when it has reached the cavity of the mouth, is maſticated and ground over again, and impregnated with a large quantity of ſaliva; this proceſs is repeated, till it becomes fit to be digeſted. I therefore entertained many doubts whether the phænomena obſerved by Reaumur and myſelf, were not rather owing to the want of rumination than of trituration. Wherefore, in order to decide with certainty concerning digeſtion in ſheep, I perceived it would be neceſſary to repeat the experiments upon plants previouſly triturated. And I did not conceive that this trituration was ſo peculiar in ruminating animals, that it could not be ſupplied by man, provided he maſticated the herbs well, and impregnated them thoroughly with ſaliva. I therefore performed this eaſy operation; the uſual tubes were employed; in three of them pieces of the green plants were encloſed, and in three others of the dried; but both had been well maſticated: the lines and nerves that traverſe the leaves, were however eaſily diſtinguiſhable. Leſt, when thus broken down and divided, they ſhould paſs out at the lateral pores, or through the meſhes of the lattice-work, I thought it would be proper to encloſe each tube in a linen bag; ſuppoſing that in the preſent caſe that it would not be broken, as that muſcular action, which is ſo conſiderable in gallinaceous fowls, does not exiſt in the animals in queſtion. I gave the ſix tubes to a ram, together with ſix others, filled with the ſame plants, but not previouſly maſticated, that I might be able to form a compariſon. Fourteen hours after the animal had taken them, he vomited three at once; and in thirty-three hours five more

were

were voided at the anus; at the end of two days he was killed. Of the four remaining tubes, two were found in the ſtomach, and the other two at the end of the duodenum; the bag in which they were encloſed was entire. The tubes that had been vomited had received more or leſs injury; the contents of two had not been maſticated, nor had they undergone the ſmalleſt alteration. The contents of the third had been maſticated, and were evidently waſted; for they now occupied little more than half the tube, whereas at firſt they had entirely filled it; they had acquired a ſubacid taſte. Many of the pieces having loſt their natural firmneſs, broke when I attempted to ſtretch them; the nerves only when they were pulled made ſome reſiſtance.

In two of the tubes voided at the anus, the pieces of plant had not been maſticated; theſe did not ſeem at all diminiſhed, or was the coheſion of the parts deſtroyed; on the contrary, the plants in the other three (which had been maſticated) were reduced almoſt to nothing; the ſmall remains conſiſted of bare nerves, with a little of the leaf attached; and both the one and the other were ſo much ſoftened, that the ſlighteſt force broke them. The bag in which they were encloſed was dyed green, particularly in its inſide; when twiſted and preſſed between the fingers, it yielded a livid yellow juice, of an acid taſte. This was far from being the caſe in the bags which contained the two tubes, of which the contents had not been chewed; their inſide had ſcarce a ſhade of green; and this ſhade was ſtill leſs perceptible in the juice expreſſed

from

from them. With reſpect to the tubes found in the laſt ſtomach, and at the end of the duodenum, the contents of the former had acquired a deep green colour, were a little macerated, but had not loſt much of their natural firmneſs, and did not appear to be diminiſhed in bulk. They had not been maſticated, but thoſe of the two others had; and of them there remained only ſome of the largeſt nerves, which were themſelves very tender, and half decompoſed. I have already obſerved, that the tubes voided at the mouth were more or leſs bruiſed; but all the reſt were quite free from injury.

CXL. The reader is already aware of the immediate conſequences of theſe experiments. In the firſt place it appears, that the gaſtric fluid of ſheep has no effect in digeſting plants, unleſs they have been previouſly maſticated; otherwiſe it can only produce a ſlight maceration, nearly as common water would do in a degree of heat ſomewhat exceeding the middle temperature of the atmoſphere. Secondly, this fluid is abundantly capable of digeſting plants, provided they are previouſly reduced to pieces by maſtication; its firſt effect is to ſoften them, and deſtroy their natural conſiſtency; it then proceeds to diſſolve them, not even ſparing the tougheſt parts, ſuch as the nerves of the leaves; of this ſolution we have a clear proof in the green colour that appears on the linen encloſing the tubes, and in the juice expreſſed from it (*a*). Thirdly, the triturating power

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(*a*) In my agreeable reſidence at Geneva in the ſummer of 1779, I had what I had long wiſhed for, the ſatisfaction power of the ſtomach does not at all contribute to digeſtion in ſheep, but this proceſs is entirely effected by the gaſtric juices. Fourthly, no ſuch power exiſts in ſheep, as we ſee from the tubes that were voided at the anus and found in the ſtomach having ſuſtained no injury, notwithſtanding preſſure alone between

faction of being perſonally acquainted with my illuſtrious friend Mr. Bonnet, and of enjoying much of his converſation. I had alſo an opportunity of taking his opinion on ſome productions, which I deſigned to publiſh, and particularly the preſent work concerning digeſtion. Three other reputable philoſophers and excellent judges of the ſubject were preſent at the reading, Mr. Abraham Trembley, Mr. Johannes his worthy nephew, and Mr. Senebier, Librarian of the Republic of Geneva; and it did not appear to me, that my labours were diſapproved by this reſpectable aſſembly. Mr. Bonnet gave me a book to peruſe on the ſame ſubject, which, as it was new to me, gave me fears, leſt the author ſhould have anticipated me; it was intituled, "Eſſai ſur la Digeſtion, & ſur les principales Cauſes de la Vigueur & de la Durée de la Vie. Par Mr. Batigne, M. D. Berlin, 1768, 12mo." But I was ſoon aware, that Mr. Batigne and myſelf had purſued very different paths; in his book he does not enter into any experimental enquiry concerning digeſtion, but confines himſelf to reflections, which, although they are very pertinent and ſenſible, are calculated rather to excite than ſatisfy the reader's curioſity. Hence I ſhould not have mentioned it, but for ſome objections ſtarted againſt Reaumur's Memoirs on Digeſtion. Theſe I ſhall touch upon in a few ſhort notes, at ſuch places of the text as they ſeem moſt connected with. And here it is proper to mention one objection relative to the digeſtion of ruminating animals, which, before I was acquainted with Mr. Batigne's book, I had myſelf urged againſt Reaumur, and which experiment proves to be perfectly juſt. It conſiſts in ſhewing, that the French naturaliſt had omitted the maſtication of the plants encloſed in tween the fingers is ſufficient to flatten them. The contuſion of the tubes that were vomited is no proof of the contrary, ſince it is evident, that this contuſion was produced by the teeth of the animal during rumination. Laſtly, theſe vegetables acquire a ſlight acidity during ſolution, but of this we ſhall have another opportunity of ſpeaking hereafter (*a*).

CXLI. This quadruped feeds not only upon graſs, but upon corn alſo whenever it meets with it; it is likewiſe very fond of bread. In order therefore to confirm ſtill farther what has been advanced, I thought it would be very proper to make an experiment upon ſome kind of grain. I ſelected wheat for this purpoſe; and as it may be procured under the various forms of ſeed, flour, and bread, I choſe to make trial of all three. Six tubes were filled, three with theſe ſubſtances without any other preparation, and three others with the ſame after they had been well maſticated. The tubes were encloſed in linen bags as before, and given to a lamb ſix months old. It was killed thirty hours afterwards; none of the tubes were either vomited or

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in the tubes before he introduced them into the ſtomachs of ſheep, which was the reaſon why they were not digeſted. Nearly the ſame objection is urged by the learned phyſician in theſe terms; "The experiments (of Mr. de Reaumur) upon ruminating animals are ſtill leſs concluſive; the graſs contained in the tubes could be only macerated, ſince it was neither chewed nor broken down a ſecond time by rumination." (L. c. Troiſieme Reflexion ſur les Experiences de Mr. de Reaumur). It was a piece of juſtice due to Mr. Batigne not to overlook this paſſage.

(*a*) In the laſt diſſertation.

voided at the anus; they were found partly in the third and partly in the fourth ſtomachs. The reſult of this experiment coincided with that related above (CXL). The grain, flour, and bread, that had not been maſticated, were indeed penetrated thoroughly by the gaſtric fluid, but not at all diſſolved. On the contrary, the corn which I had firſt bruiſed with a peſtle and then ground between my teeth and reduced to a coarſe paſte, was in great meaſure conſumed; nothing remained in the tubes but fragments of the bran, with ſome ſmall remains of farinaceous matter adhering. The like had happened to the flour and bread, what remained conſiſted of a mucilaginous maſs, without any appearance of what it had originally been. This matter had a ſlight degree of acidity, a quality which was far more evident in the bread, flour, and grain that were not diſſolved by the gaſtric fluid for want of previous maceration.

CXLII. The vaſt quantity of gaſtric fluid with which ruminating animals are continually ſupplied, was already known to phyſiologiſts, and particularly to the great Haller. After a faſt of two whole days, I have found thirty-ſeven ounces in the two firſt ſtomachs of a ſheep. It was green, but I know not whether this colour is natural to the fluid, as the yellow hue to that of crows (LXXXI); or rather whether it is adventitious, and derives its origin from the plants on which theſe animals feed, of which, notwithſtanding ſo long a faſt, there were ſtill ſome remains in the two ſtomachs. The great quantity of juice I had collected induced me to try whether, like that of ſeveral other animals,

mals, it was capable of digeſting food out of the body. I therefore encloſed ſeveral pieces of the leaves of lettuce in two ſhort glaſs tubes (which I had previouſly filled with the juice), and ſealed them with wax at each end. The contents of one tube, as before, were maſticated, while thoſe of the other were left untouched. It was proper on the preſent occaſion to employ a term of compariſon, by repeating the ſame experiment upon two other tubes filled with water. That theſe four tubes might be expoſed to a degree of heat nearly equal to the temperature of ſheep, I fixed them under my axillæ, two under each axillæ, where they continued forty-five hours. The leaves immerſed in the gaſtric fluid, which had been previouſly macerated, had undergone no inconſiderable change. Beſides the loſs of their bright green colour, they were converted into a kind of glue, in which it was juſt poſſible to find, with the point of a penknife, a few nerves, which were the only remains of the organization of the plant. This was far from being the caſe with the leaves that had not been maſticated; for all the pieces were diſtinguiſhable, and the only difference was, that they did not afford ſo much reſiſtance as at firſt. The leaves immerſed in water, both thoſe which had been chewed, and thoſe which had not, had not loſt either their colour or conſiſtence. From this compariſon it appears, that the gaſtric fluid does not act on the plant as a mere aqueous fluid, but as a real ſolvent, nearly as it acts in the ſtomach itſelf. Nor was the heat to which it was expoſed under my axillæ a condition without its part in the production of this incipient digeſtion; for in pieces of the ſame leaves of lettuce maſticated in the ſame manner, but kept in my apartment, of which the temperature was about ſixteen * deg. there appeared only a ſuperficial maceration, notwithſtanding they remained immerſed in the ſame gaſtric fluid for the ſame ſpace of time.

CXLIII. I cloſed my enquiries concerning the digeſtion of ruminating animals by ſome experiments on oxen. In theſe the ſame tubes and plants were employed as before, and the reſults perfectly coincided with thoſe obtained from ſheep; only in the preſent inſtance, Nature was more ſpeedy in her operation. Before twenty-four hours had elapſed, the tubes, which had been given to two oxen, were voided along with the excrements; they were not in the leaſt contuſed or injured. When taken out of the linen bags and examined, they were found to contain little more than the bare ribs and nerves of the leaves of beet, lettuce, and trefoil (which leaves had been previouſly maſticated). The nerves were alſo in ſome degree macerated, and the ſlighteſt violence was ſufficient to break them. On the contrary, pieces of the ſame plants that had not been ſubjected to maceration were indeed ſlightly concocted, and their colour was a little faded, but they were entire. When applied to the tongue, they taſted ſubacid, like thoſe which had been in the ſtomachs of ſheep (CXXXIX, CXLI).

The horſe does not chew the cud, but he reſembles the ox in the membranous ſtruc-

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* Sixty-ſix, Fahren.

ture of his ſtomach, and the food upon which he lives. I was therefore deſirous of ſeeing what changes maſticated plants would undergo by continuing a given time in the ſtomach of this quadruped alſo, encloſed as uſual in tubes. Here too they were digeſted, as I learned from ſome lettuce and trefoil encloſed in two tubes, which were voided in fifty-two hours.

CXLIV. When I reflect upon the various animals to which my enquiries concerning digeſtion have been hitherto extended, I perceive, that the ruminating ſpecies very nearly reſemble birds endowed with muſcular ſtomachs, with reſpect to the action of the gaſtric fluid. In both, that fluid requires an agent capable of breaking down and triturating the food, before it can diſſolve and digeſt it. From the mouth of granivorous birds, where it undergoes no real alteration, the aliment paſſes immediately into the craw, where it is ſoftened and macerated; from this receptacle it deſcends into the ſtomach: the triturating power of this organ performs the office of teeth, and breaks, grinds, and, if I may ſo ſpeak, pulverizes it, and thus renders it fit to be diſſolved by the gaſtric fluid, and converted into chyme. Nature employs a ſimilar contrivance in ruminating animals. The hay and graſs deſcend immediately into the firſt and ſecond ſtomachs, in nearly the ſame ſtate as when they were browſed. Here they are ſoftened by the exuberance of gaſtric juices, as ſeeds in the craw of birds with gizzards. But as the ſtomachs of ruminating quadrupeds have no ſenſible triturating power (CXXXIX, CXL, CXLIII),

CXLIII), and the aliment requires trituration, nature has wiſely provided for this by cauſing it to aſcend, in conſequence of a gentle ſtimulus to vomit, into the cavity of the mouth, where, by means of rumination, it receives the neceſſary prediſpoſition to be digeſted by the gaſtric fluid, as happens to the food in the ſtomachs of granivorous fowls, after they have been properly triturated by the gaſtric muſcles.

DISSERTATION IV.

THE SUBJECT OF DIGESTION IN ANIMALS WITH MEMBRANOUS STOMACHS CONTINUED. THE LITTLE OWL. THE SCREECH OWL. THE FALCON. THE EAGLE.

CXLV. REAUMUR having treated in his firſt memoir of the mode of digeſtion in granivorous and herbivorous fowls which are provided with gizzards, in his ſecond proceeds to enquire into the nature of that function in carnivorous birds, of which the ſtomach is membranous. From the facts related in the firſt memoir he concludes, that there does not exiſt in the gizzard any ſolvent capable of ſeparating the particles of the food. This ſeparation, he thinks, is effected by a force reſembling that exerted by mill-ſtones, viz. the action of ſtomachs of this conſtruction upon their contents. He is moreover of opinion, that the facts adduced in the ſecond memoir prove the exiſtence of a menſtruum in membranous ſtomachs, capable of diſſolving and digeſting the aliment without borrowing any aid from the action of the ſolid parts.

As the great object of the firſt diſſertation was to enquire experimentally into the mode of digeſtion in fowls with muſcular ſtomachs, I had there an opportunity of conſidering fully Reaumur's experiments on that ſubject; and we have accordingly ſeen, that the conſequences he has deduced from them are by no means to be admitted in their full extent. This is plain from the XXXIX, XL, XLI, XLII, XLIII, XLVth paragraphs, to which, for the ſake of avoiding uſeleſs repetitions, I refer the reader. The preſent diſſertation, in which the ſubject of digeſtion in membranous ſtomachs is continued, is the proper place for conſidering the experiments related in the ſecond memoir. As of all fowls birds of prey approach neareſt to man in the ſtructure of the ſtomach, he choſe one of thoſe large kites that are common in France, for the ſubject of his enquiries. The periodical vomiting, common to all birds of prey, allowed the French naturaliſt to make a variety of experiments on the ſame individual. He employed tin tubes filled with different ſubſtances, eſpecially fleſh, which, after having been ſome time in the ſtomach, were thrown up, and gave him an opportunity to examine the effects produced upon the contents. That the fleſh was more or leſs digeſted according to the length of its continuance in the body of the animal, was the general and invariable reſult obſerved by Reaumur (*a*). Hence he juſtly infers, that in this caſe digeſtion is produced by the gaſtric fluid, without the concurrence of any friturating power. He mentions ſome other experiments, which I ſhall have occaſion to conſider below, and concludes from analogy, that digeſtion in other birds with membranous ſtomachs is produced in the ſame manner. He laments however, that from the death of his kite, and his neglecting to ſubſtitute other animals in its ſtead, he could not adduce facts ſufficiently numerous to illuſtrate the ſubject fully. He promiſes to ſupply the deficiency on ſome future occaſion, but his death, by which a few years afterwards natural philoſophy loſt its great

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(*a*) Mr. Batigne thinks, that fleſh encloſed in tubes was inſufficient to convey a preciſe idea of the alteration it undergoes in the ſtomach, as it is only macerated in tubes, and not digeſted. "On voit de plus que la viande miſe dans les tubes ne peut donner une idée preciſe des changements qu'elle ſubit dans l'eſtomac de l'animal, puis qu'elle n'y eſt que macerée & non point digerée." (*L. c. premiere Reflection ſur les experiences de M. de Reaumur.*) The author muſt allow me to obſerve, that in this attack he miſrepreſents Reaumur, who, p. 465, &c. of the Mem. of the Roy. Acad. expreſsly ſays, that the fleſh given to the kite was not merely macerated or ſoftened, but completely digeſted, and at laſt entirely conſumed. He might indeed have objected to the ſmall number of his experiments, as inſufficient to aſcertain the efficient cauſe of digeſtion, if that philoſopher, whoſe ingenuouſneſs was equal to his ſkill, had not perceived and publicly owned it himſelf. That tubes, provided the experiments are properly made and varied, are well adapted to ſhew the change produced upon food in the ſtomach, will be abundantly proved by the facts adduced in this treatiſe.

ornament, prevented him from fulfilling his promiſe.

CXLVI. I do not preſume that I ſhall be able to accompliſh, what neither this illuſtrious naturaliſt, nor any other, as far as I know, has effected. But ſimply with the view of continuing my obſervations and reflections on digeſtion in fowls with membranous ſtomachs, I ſhall relate ſome experiments on various birds of prey, of which ſome ſeek it by night, and others by day. Among the former, I have uſed ſuch as I could moſt eaſily procure, the little owl and the ſcreech-owl. The food which I gave the firſt-mentioned ſpecies (*a*), and which it eagerly devoured, has enabled me to ſolve, among other problems, one that exerciſed the ſagacity of M. de Reaumur. Finding that the gaſtric fluid of the kite digeſted fleſh, he wiſhed to know whether it would alſo digeſt vegetables; a circumſtance he did not think probable, when he conſidered the repugnance carnivorous birds ſhew for them; and ſo in fact it happened. When beans, peaſe, wheat, incloſed in tubes, had lain ſome time in the ſtomach, they were thrown up juſt in the ſame ſtate as they had been ſwallowed: nor did boiling diſpoſe them to be diſſolved any better by the gaſtric fluid. Some ſparrows, which I gave my owls, afforded me an opportunity of obſerving the ſame phænomenon. As they ſwallowed them whole, they of courſe would receive into the ſtomach feathers and food

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(*a*) This ſpecies is called by Buffon *petite chouette*, Hiſt. Nat. des Oiſeaux, T. 2. Ed. in 8vo. and by Linnæus *ſtrix paſſerina*, l. c.

not yet digeſted by the ſparrows, and conſiſting of grain or bread. Now, after the fleſh has been digeſted, the feathers are vomited generally in the form of a hard ball; and along with the feathers the grain, which, though it is much ſoftened by maceration, yet continues whole. And if the matted feathers be diſentangled, we may generally perceive evident traces of bread. Hence we have a clear proof that the gaſtric fluid produces no change on ſuch vegetables.

CXLVII. This fact, ſimple as it is, ſhews two things, of ſome importance to be noticed: firſt, that the ſtomach of this bird is really membranous, and without any power of trituration: this appears from the grains (CXLVI) continuing whole, though they had been ſoaked till they were become ſo tender, as to burſt on being gently ſqueezed between the fingers. I would not however alledge that the ſtomach has no action at all; for the globular mats of feathers can only be produced by this viſcus contracting as the fleſh is digeſted.

The digeſtion of the bones alſo deſerves attention. It cannot be ſaid that they are voided along with the excrements; for I muſt ſoon have been aware of this, as I kept my owls in cages; nor, for the ſame reaſon, could it have eſcaped my notice if they had been vomited. I have indeed ſometimes found two or three little bones, as a dorſal vertebra, or a piece of the cranium, among the matted feathers, but never any thing like the whole ſkeleton. We muſt therefore conclude that they are digeſted.

CXLVIII. Reaumur's kite was capable of digeſting bone, though of the hardeſt texture, and encloſed in tubes (*a*). Though the experiment juſt related is ſufficiently deciſive, yet, as the bones were looſe in the ſtomach, to be abſolutely certain that the effect was produced by the gaſtric fluid alone, it was proper to repeat it with a tube; with this view a piece of the thigh of a pigeon was put into one of the ſame tubes that I uſed before: thus two experiments, one on the digeſtion of fleſh, and another on that of bone, were made at once. By long practice upon birds of prey, I learned how to keep the tubes in the ſtomach as long as I pleaſed. When I had given one of my tubes to an owl, after it had been full fed, I found it was not thrown up till all the food was digeſted. This obſervation is applicable to all other birds of prey. The ſame thing alſo happened when they were fed ſparingly. All the difference was, that as the full ſtomach requires more time to be emptied, the tubes were retained longer, and *vice verſa*. When they were faſting, the tubes were ſure to be returned in two or three hours. This obſervation, together with the knowledge I had acquired from experience, of the time theſe birds take to digeſt a given quantity of food, enabled me to gueſs pretty exactly how long the tubes would continue in the ſtomach.

I return now to the tube in which part of a pigeon's thigh had been put. After ſeventeen hours continuance in the ſtomach, the bone was no where changed except at the

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(*a*) Mem. cit.

broken ends, which were a little ſoftened. The fleſh, by which it was covered, as well as the integuments, had begun to be diſſolved, for the ſurface was become exceedingly tender. In fourteen hours more greater effects were produced. The fleſh was conſiderably waſted; the bone was ſhortened at the ends, and was ſo ſoft, as to yield to the preſſure of the finger. In twenty-ſeven hours more, there was no remains of fleſh or perioſteum, and the bone was a good deal ſhorter than at firſt. I could not but be deſirous of ſeeing the end of the experiment, and therefore replaced the bare bone in the tube. When it had remained twenty-one hours in the ſtomach, it had loſt the marrow, and the internal cavity was enlarged, though the girt was leſſened. This aroſe from the corroſion of the internal and external ſurfaces at the ſame time. Both ſurfaces were covered with a yellow fluid, that had at once a bitter and ſalt taſte; and points of gelatinous matter were diſperſed over them. The bone, thus half diſſolved, was put again into the tube, and left thirty-two hours longer in the ſtomach. If the reader can conceive a cylinder of thin paper, uneven at the ends, and perforated with ſeveral holes, he will have an idea of the ſtate of the bone when it was taken out of the tube. It was covered with the ſame fluid, which muſt have been the gaſtric liquor; and the gelatinous points now alſo were diſperſed over the ſurface of the leaf; this jelly was the oſſeous matter itſelf, reduced to this ſtate by the action of the gaſtric fluid. Laſtly, nine hours longer continuance in the ſtomach, left only a few ſmall chips. This one experiment convinced me, that the gaſtric fluid of the little owl is capable of digeſting bone as well as fleſh, without the concurrence of any external agent: it alſo ſhews the gradual progreſs of digeſtion.

CXLIX. Having ſo far ſatisfied my curioſity, it remained to enquire into the nature of this fluid, and its effects out of the animal body. With the ſmall ſpunges, by means of which I obtained ſo large a quantity from crows (LXXXI, LXXXII), I procured it in due proportion from the ſpecies of owl in queſtion. I ſay in *due proportion*; for it is evident, that as the ſtomach of theſe birds will not admit ſo many tubes as that of crows, it cannot yield ſo much gaſtric fluid. Beſides, I had only ſix little owls, whereas I could get as many crows as I pleaſed. It was wonderful how ſoon the ſpunges were filled with liquor. As they were introduced into the empty ſtomach, they were ſoon thrown up, agreeably to an obſervation made above (CXLVII); and yet they were as full of juice, as if they had been dipped in water; and freſh ones immediately forced down the throat, yielded a nearly equal quantity. I obſerved the ſame thing in crows (LXXXIII). Whence it appears, with what care Nature provides a large ſupply of gaſtric liquor in theſe animals, as digeſtion is entirely dependent upon it. The juice was inſtantly ſqueezed out of the ſpunges into a ſmall glaſs; it appeared to have the fluidity of water, but was of a reddiſh-yellow colour, like the yolk of an egg. This colour was not inherent in the gaſtric liquor; it aroſe from an immenſe number of very ſmall yellow corpuſcles, ſcarce perceptible by the naked eye, but eaſily ſeen by help of the microſcope. In a few hours they ſubſided to the bottom, in the form of a yellow ſediment, and left the fluid above tranſparent, like water, where it has been freed from mud that was diffuſed through it, and rendered it turbid. The firſt time I ſaw this phænomenon, I ſuſpected it was owing to ſome impurities that remained in the ſtomach, and were mixed with the juice. Before the next experiment, in order to be certain that the ſtomach was free from heterogeneous ſubſtances, I kept the animal faſting for a longer time than uſual; but this did not prevent the yellow colour from appearing. Upon opening the ſtomach of an owl that had been long kept faſting, I could find no foreign ſubſtance, but the fluid was as yellow as that ſqueezed out of the ſpunges. I was therefore convinced that theſe particles, though I could not diſcover their origin, did not come from any remains of the food. The gaſtric liquor of the little crow, like other gaſtric liquors, is a little ſalt and bitter: it evaporated ſooner than water. It leaves a ſediment of the yellow particles; which gradually becomes dry, and forms a blueiſh yellow cruſt; it is not at all inflammable. It has one property common to every gaſtric fluid I have hitherto examined, or ſhall have occaſion to mention in the ſequel; though it is expoſed to the open air for weeks and months, in the hotteſt ſeaſon, it never becomes putrid.

CL. Such are the properties of the gaſtric liquor of the ſmall owl, when examined alone. Let us proceed to the effects it produces on fleſh out of the body. In theſe experiments I uſed calves inteſtines, a kind of food which this bird devours very greedily. Forty-ſix grains were immerſed in ſome recent gaſtric fluid; and at the ſame time an equal quantity of the ſame inteſtine was put into a phial exactly like the former, and an equal quantity of water was poured upon it. Whenever I have made experiments, with a view to compare the effects of the gaſtric liquor and water, I have taken care that all circumſtances ſhould be alike. To prevent evaporation, the mouths of the phials were ſtopped with paper; they were ſet near a kitchen fire, where the uſual heat was between thirty and thirty-five degrees. In eleven hours ſome black ſpots began to appear upon the inteſtine in the gaſtric fluid, which were at firſt thinly ſcattered over it, but became gradually more numerous, till in twenty-four hours they almoſt covered it. During the formation of the ſpots, I examined the inteſtine with the microſcope, and found that where they appeared, the fleſh was ſoftened, and had loſt its fibrous texture. When they had ſpread over the whole piece, I took it out of the liquor, and waſhed it with pure water; and now it recovered its white colour, for the black covering conſiſted of a thin ſtratum of fleſh, which the gaſtric fluid had concocted. It was very eaſily rubbed off, and fell to the bottom of the water in exceedingly ſmall particles, where it formed a black ſediment, and when viewed by the microſcope, ſeemed to be a collection of molecules of fleſh, with no appearance of fibres.

When the piece of gut was dried, it weighed only twenty-eight grains, and had therefore loſt eighteen; the piece that had ſtood in water for the ſame length of time, was quite fœtid; whereas the other emitted no diſagreeable ſmell: after waſhing and drying, it was found to have loſt ſeven grains. Both pieces were again put into the phials, with the ſame quantity of water and gaſtric fluid, and left in their former ſituation for two days. The latter had now loſt its ſhape and organization, and was converted into a black mucilage, of which the particles had no longer any coheſion. The gaſtric liquor had therefore diſſolved the piece of inteſtine completely; an effect, which neither water nor putrefaction had produced upon the other; for there was a remainder of nineteen grains, that not only retained its fibrous ſtructure, but made conſiderable reſiſtance when I attempted to tear it.

CLI. I did not neglect to examine the ſtomach and œſophagus of this ſpecies of owl, as I conceived that it would be improper to omit a brief deſcription of theſe organs in the animals upon which my experiments were made. If the beginning of the duodenum be tied, ſo as to ſtop the air from paſſing, and the upper end of the œſophagus be inflated, we get a view of the œſophagus and ſtomach dilated to their utmoſt extent; together they reſemble a pear, or rather a gourd, of which the belly is formed by the ſtomach, and the neck by the œſophagus; when inſpected againſt the light, the latter appears ſemi-tranſparent, and the former quite opake. If they are cut longitudinally,

tudinally, and ſpread upon a table, we find that the tranſparency of the œſophagus is owing to the thinneſs of its ſides, which thicken as they deſcend, and render the lower part as opake as the ſtomach. It becomes not gradually, but ſuddenly thicker, from the multitude of follicular glands that form the ſame kind of tranſverſe faſcia, that I have deſcribed in other birds; in this ſpecies it is about five lines broad. Theſe glands continually ſecrete into the cavity of the œſophagus a liquor almoſt inſipid, of a turbid white colour, and of ſome denſity; in a word, reſembling the œſophageal juice of other birds. At the beginning of the ſtomach the follicles diſappear, nor could I find the ſmalleſt veſtige of any thing like them in the coats, though I ſearched with care. Are we then to ſuppoſe that the ſource of the fluid, which is always to be found in the ſtomach, is from the numberleſs glands lying at the bottom of the œſophagus; this is probably true of part; but that no ſmall part comes alſo from the arteries of the ſtomach itſelf, the humidity, like what I have deſcribed in other animals (XCIII, CXXXII), has furniſhed me with an indubitable proof; for it immediately appears again, after it has been wiped off ever ſo clean.

CLII. This deſcription will apply to the œſophagus and ſtomach of ſcreech-owls: I have made experiments on two ſpecies of the owl; one variegated with many colours, among which the red and brown, or dull yellow, predominate; upon the head are two curious tufts, in the ſhape of a creſcent; the other ſpecies has not this tuft, but is adorned with a greater variety of elegant colours: the iris is duſky, in the former it is yellow (*a*). My firſt experiment was made upon one of the long-eared owls, and the reſult greatly ſurprized me. It threw up two tubes in about three hours after it had taken them, nor was the fleſh at all changed; I could not perceive any alteration, even when it had continued upwards of ſeven hours in the ſtomach. If I had not been very cautious in forming opinions, I ſhould have concluded, that the gaſtric juices of this ſpecies are inſufficient of themſelves to produce digeſtion; but I reflected, that a ſingle experiment did not warrant ſuch a concluſion, and that ſome adventitious circumſtance might have affected the reſult. The bird ſeemed quite ſtupid, and reduced very much in its fleſh; hence it was probably unhealthy, and conſequently incapable of digeſting its food properly. This ſuſpicion was confirmed by the account of the perſon from whom I had it, who informed me, that it had refuſed food ever ſince it was taken, which was now four days. It was an old bird; and, upon turning to Buffon, I found that, in order to rear individuals of this ſpecies, it is neceſſary to catch them young, for the old ones will not take ſuſtenance in confinement (*b*). In two days and a half longer, that in my poſſeſſion

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(*a*) The former ſpecies is called by Linnæus *ſtrix otus*, and *moyen duc* by Buffon; the latter *ſtrix ſtudula* and *chat buant*.

(*b*) A. l. c.

died without taking any food, and returning what I forced down the throat.

CLIII. This owl fell into my hands in winter; the ſpring following I procured two young ones from the neſt, which devoured food with eagerneſs whenever hunger preſſed upon them: I now repeated my experiment, and the reſult was exactly the reverſe of the preceding; the fleſh in the tubes ſhewed ſigns of ſolution in three hours and three quarters, and in ſeven was entirely diſſolved. This convinced me, that the failure of the foregoing experiment was not owing to the inefficacy of the gaſtric fluid, but to the morbid condition of the animal; which either leſſened its quantity, or, what is more probable, impaired its quality. I might, therefore, have omitted mentioning that failure; but it was better to relate it, in order to ſhew, that when the food incloſed in tubes is not digeſted, we are not immediately to infer, that the gaſtric fluid is not capable of producing this effect.

CLIV. But my young owls digeſted not only fleſh, but bone; and that of a hard texture, ſuch as the bones of ſheep and oxen, not to mention thoſe of pigeons and fowls. The reſult was eſſentially the ſame as in the preceding ſpecies (CXLVII, CXLVIII); inſtead, therefore, of dwelling upon it, I will relate, at ſome length, a fact, which, in my opinion, deſerves to be noticed. I gave one of my owls a frog, and in an hour killed it. The ſtomach was exceedingly dilated, and was incapable of containing the whole frog, of which the head lay in the œſophagus, and ſtretched the ſides conſiderably; the hind legs lay at the bottom of the ſtomach, and the fleſh was ſo much waſted, that the bones were nearly bare: the integuments of the thighs and trunk were almoſt corroded, and the fleſh was as tender as if it had been boiled. The head, which lay contiguous to the faſcia of follicular glands at the bottom of the œſophagus, had begun to be diſſolved. This experiment ſhews not only that fleſh is digeſted with great quickneſs by the gaſtric liquor, but likewiſe that it is digeſted equally ſoon in the œſophagus and ſtomach; an obſervation I had not yet made upon any other animal.

CLV. Before I killed both theſe owls, I was deſirous of having ſome of their gaſtric juice, that I might ſee whether it retained like others its power of digeſtion; and I found, that fleſh is completely diſſolved by it even when it is aſſiſted by a proper degree of heat.

CLVI. In the other ſpecies, the *tawny owl*, the ſame phænomena occurred with reſpect to the ſolution of fleſh and bone in the tubes, whether we conſider the digeſtion of fleſh and bone in tubes, or the ſpeedy digeſtion in the œſophagus (*a*), or the remarkable ſlowneſs

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(*a*) When I was compoſing the text I was ſtruck by a reflection, for the inſertion of which this is the proper place. If we compare the preſent with the LXXVII, LXXVIII, LXXIX, XCIX, Cth, CIſt, CXXXV, CLIVth, it will appear, that the œſophageal before its mixture with the gaſtric fluid, in many animals, is endowed with ſome degree of digeſtive power. This virtue, though it is generally only exerted in conjunction with the gaſtric fluid, yet in ſeveral animals produces its effects in the œſophagus, as we have ſeen on more than one occaſion, when the ſubject was animals of which the ſtomach is not capacious enough to hold all the food (which is ſwallowed with great eagerneſs), and part muſt therefore be lodged in the œſophagus.

of that proceſs out of the body. Upon an individual of this ſpecies I made an experiment, which had been unſucceſsful on the *little* owl. Obſerving, that when they were hungry and open their beak very wide, if I dropped a pea, French-bean, or cherry into it they ſwallowed it with as much avidity as if it had been the pleaſanteſt kind of food, I was deſirous of ſeeing whether the ſtomach would digeſt vegetable ſubſtances. With this view I encloſed ſome of the ſeeds juſt enumerated in ſome tubes, and forced them down the throat, but to no purpoſe; for though the liquor ſwelled them, and perhaps altered the colour, they underwent no diminution of bulk. They were thrown up undigeſted in a day or two, a circumſtance which ſufficiently ſhews, that ſuch kind of food, notwithſtanding they appear to reliſh it, is ill adapted to their gaſtric juices. The greedineſs with which they ſwallow ſuch ſubſtances can ariſe only from that blind appetite, in conſequence of which birds take whatever is offered them.

CLVII. Being ſatisfied with theſe experiments on nocturnal birds of prey, I turned my attention to ſome of the diurnal ones. My firſt ſubject was a falcon given me by my illuſtrious friend the abbe Corti, formerly profeſſor of natural hiſtory at Reggio, and now ſuperior of the College of Nobles at Modena, a philoſopher well known in the republic of letters by ſeveral fine publications. It was of the ſize of a common hen, and appeared to belong to the ſpecies denominated *lanarius* by Linnæus. I ſoon found, that I could not handle this bird ſo familiarly as thoſe which I have had occaſion to mention hitherto. The ſtrong beak and long ſharp talons would not eaſily permit me to open the mouth by force, and thruſt the tubes down the throat. I however contrived a method of introducing them into the ſtomach without the bird being aware; it conſiſted in cutting ſome fleſh in pieces, making holes in them, and concealing the tubes in theſe holes. When the falcon was hungry he ran eagerly to the pieces of fleſh, and ſwallowed them whole. For the fraud to ſucceed, it was neceſſary that the tubes ſhould be quite covered with fleſh; for if any part of them was bare, the falcon would put them under his talons and tear the fleſh away with his beak and ſwallow it, leaving the tubes.

CLVIII. My firſt experiment was made with a view to aſcertain, whether it was capable of digeſting bone independently of the action of the ſtomach. The reſult was ſucceſsful; but I have before ſaid ſo much on the ſubject of the digeſtion of bone, that I ſhould omit relating the preſent inſtance particularly but for a new and important phænomenon, which renders the detail neceſſary. The bone conſiſted of little ſplinters of an ox's thigh bone; they were very hard and compact, and of various ſizes, from a grain of wheat to a bean; they weighed together ſixty-ſeven grains; I put them into two tubes, in which they were rather cloſely crammed. To prevent their falling out of the tubes when they began to be diſſolved, and conſequently to get looſe from each other, I put the tubes in a linen bag, a precaution which I had before employed, and continued to employ occaſionally in future. In twenty-four hours the bones had ſhifted their reſpective places and rattled in the tubes, a circumſtance that ſhewed the bulk to be diminiſhed. They were moiſt with gaſtric liquor, but had none of thoſe gelatinous points which I had ſeen in an experiment both on the *little* owl (CXLVIII), and the two other ſpecies. Theſe points were, as I then remarked, the oſſeous matter converted into jelly or chyme by the gaſtric liquor. But what is extrordinary was, that theſe ſplinters retained their original hardneſs and rigidity; ſo that at firſt ſight one would not have ſuppoſed, that the fluid of the ſtomach had had any effect upon them. However, the contrary was certain; for when the gaſtric liquor was wiped off, they weighed only forty-two grains. I now replaced them in the tubes, and examined them again after they had been two days in the ſtomach. The pieces of the ſize of grains of wheat were all deſtroyed but two, which were now no larger than millet. Three of the ſplinters were at firſt as big as beans, but now reduced to the ſize of maize. Thoſe of an intermediate ſize were diminiſhed in proportion. During the whole time they all continued hard. At the third examination, after fifty-ſeven hours longer continuance in the ſtomach, the three large pieces only were left, and they were now no larger than millet; when I ſtruck them with an hammer, I found that they retained their original hardneſs.

The gaſtric liquor therefore of the falcon does not, like that of owls and many other animals, inſinuate itſelf into the ſubſtance of the bone, but acts on the ſurface only. The phænomenon, I think, may be thus explained: conceive a bone to be compoſed, like wood, or to bring a more familiar inſtance, like an onion, of a great number of ſtrata. The ſtrata of the onion are of conſiderable thickneſs, but we muſt imagine, that in bone they are exceedingly thin. The gaſtric fluid of owls or other animals will firſt diſſolve the upper ſtratum, but while it is doing this it will penetrate and ſoften the contiguous ſtrata, without diſſolving them. Hence the tenderneſs of bone that has lain in the ſtomachs of animals. On the contrary, we muſt ſuppoſe, that the gaſtric liquor of the falcon has no power of penetrating the internal ſtrata, but that its action is limited to the ſurface. According to this ſuppoſition the bone will be digeſted without having the internal parts ſoftened, and thus ſtratum after ſtratum will be taken away, juſt as it would happen if we had a menſtruum capable of diſſolving only the ſuperficial layer of an onion without acting upon the others.

CLIX. Before I concluded poſitively that the gaſtric juice does not ſoften bone at all, I determined to obſerve its effects when it is at liberty to act without any obſtacle; for it is poſſible, that its efficacy might be impaired by paſſing through the cloth. I thereſore took a piece of the ſame thigh bone from the thickeſt part, and worked it into a ſphere by the lathe, to prevent the angles injuring the fine coats of the ſtomach; it was then given to the falcon. My purpoſe was to obſerve whether as it was diſſolved it was alſo ſoftened.

It continued five days in the ſtomach without becoming in the leaſt tenderer. The ſhortening of its diameter ſhewed that it was leſſened in bulk. Meantime the falcon threw up the ſphere once or twice a day, according as he was ſupplied with food; for, as I have obſerved with reſpect to other birds of the ſame claſs (CXLVIII), he did not vomit indigeſtible bodies till he had digeſted the other contents of the ſtomach. To cauſe indigeſtible ſubſtances to remain in that cavity after digeſtion was over, I gave him freſh food; for experience had taught me to judge, when that period was approaching, I was ſure to attain my purpoſe; ſince when the crop is full of food, the contents of the ſtomach cannot be evacuated through the mouth. By this contrivance the falcon was made to retain the globe twenty-two ſucceſſive days. It is ſcarce worth while to obſerve, that it was not ſoftened, ſince the inability of the gaſtric fluid to produce this effect has been ſufficiently proved before; but the remarkable diminution it underwent deſerves to be noticed. The ſphere was at firſt four lines and an half in diameter, and when it had been thirty-five days and ſeven hours in the ſtomach it meaſured only a line and about a third; it preſerved its form perfectly; the ſame may be ſaid of its poliſh; there was not a furrow, nor an indentation, nor an aſperity of any ſort upon the ſurface. This ſmoothneſs is, I think, a clear proof, that the ſtomach of this ſpecies has no triturating power, otherwiſe the globe would have ſuſtained ſome injury from the friction and impulſes of ſo many tin tubes as were introduced into the ſtomach during its continuance there.

CLX. Let it not however be imagined, that bones of a texture leſs rigid require ſo much time to be diſſolved; this was very far from being the caſe. My falcon would eat a whole pigeon at once, for birds of this kind always when they take any large prey fill themſelves quite full, and then continue ſeveral days without food. My falcon refuſed the entrails, the tips of the wings, and the beak; the reſt he devoured with the utmoſt greedineſs. But no bone or fleſh was ever vomited, nor did any thing paſs out at the vent in the form of bone or fleſh; the excrements now, as well as at other times, conſiſting of a ſemifluid matter, partly white and partly black. When dry it might be reduced to an impalpable powder by rubbing between the fingers. This animal therefore digeſted not only the fleſh, but the bones of a pigeon, and that in the ſhort ſpace of a day; for at the expiration of this time it would eat a ſecond pigeon.

CLXI. While I was examining the manner in which the falcon digeſts bone, I was ſtruck with a thought that had never occurred to me during the whole train of the foregoing experiments; it was to enquire whether the gaſtric liquor beſides bone is alſo capable of digeſting ſome other animal ſubſtances, ſuch as the enamel of the teeth, the tougheſt tendons, and horn. With this view I encloſed two inciſors from the lower jaw of a ſheep in a tube, which the falcon retained three days and ſeven hours. Wherever the enamel did not extend they were corroded and waſted, but the other parts were uninjured, and as brilliant as at firſt. In four days and an half longer continuance in the ſtomach the fang was nearly diſſolved, but the enamel was perfectly ſound. The teeth were kept two days more in the ſtomach without the tubes, but no further effect was produced; whence it was neceſſary to infer, that the gaſtric juice of the falcon is incapable of diſſolving the enamel of the teeth; a circumſtance which is not very ſurpriſing, ſince it differs from every other oſſeous ſubſtance.

CLXII. I have elſewhere obſerved, that birds of prey, and conſequently falcons, vomit the feathers of the birds which they eat (LIX); it is therefore evident, that the gaſtric fluid cannot digeſt them. The ſmell emitted by burning feathers ſhews, that they approach the nature of horn; it was therefore reaſonable to ſuſpect, that corneous ſubſtances would not be diſſolved in the ſtomach, a ſuſpicion which was verified by the event. Some pieces of ox's and ſheep's horn were as uſual concealed in fleſh, and given to the falcon. In a few days they were thrown up entire and uninjured. I have remarked, that the internal coat of the ſtomach in gallinaceous fowls is not tender and yielding, as in many animals, but firm and cartilaginous (XXXV, XLVII, XLIX, L). Having frequently obſerved, that when burned it exhales an odour very much like feathers and horn, I ſuppoſed that it would in like manner elude the action of the gaſtric fluid, which really happened; and not only in the thick coats of turkeys and geeſe, but in the

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thin ones of pigeons, blackbirds, and quails. When I gave my falcon the whole ſtomach of any of theſe fowls, the other coats were ſoon digeſted, but the cartilaginous remained entire.

In tendons the reſult was different; for my experiment I choſe an ox's *tendo achillis*, one of the moſt tenacious tendons that is to be found in animal bodies. It was hung to dry in ſummer for ſeveral weeks, and thus became ſo hard, that a keen knife would hardly cut it. However, the gaſtric liquor of the falcon diſſolved it both when it was encloſed in tubes, and looſe in the ſtomach.

CLXIII. Moſt ſhoes have the upper leather of calf-ſkin, and the ſole of ox's hide. Both theſe ſubſtances are very readily digeſted by carnivorous animals when freſh: this at leaſt is the caſe with the falcon; but the contrary happens when they have been tanned. Another fact has warned me how cautious we ought to be in forming general rules in phyſics. Who would not have concluded from the laſt experiment, that every other kind of leather is alſo indigeſtible? Yet the reverſe happened in ſheep-ſkin dreſſed, and dyed yellow. Some liſts of it were encloſed in tubes, and completely digeſted in ſeven hours.

CLXIV. As I had found the gaſtric fluid of other carnivorous animals incapable of digeſting vegetable matters, it was more than probable, that the ſame thing would take place in the falcon. I however thought, that it would be proper to aſcertain this point by experiment, if for no other reaſon, yet on account of the recent inſtance of the uncertainty of analogical arguments (CXLIII). At the ſame time I was deſirous of determining whether digeſtion is the effect of the gaſtric liquor ſolely, as it ſeemed more than probable. The falcon could very well take ſix tubes at a time: four were filled with various vegetable ſubſtances, ſuch as crumb of bread, chick-peaſe, ſlices of pears and apples; in the fifth and ſixth were encloſed mutton and beef. Upon theſe ſubſtances the effects of the gaſtric fluid were exactly the reverſe. The fleſh was totally diſſolved in twenty-ſeven hours, but the vegetables had undergone no alteration. Two freſh tubes, containing in the middle a bit of fleſh, and at the ſides maſticated bread and boiled peaſe and chick-peaſe, decided the queſtion ſtill more clearly. The vegetables were undiminiſhed, but the fleſh, which was ſurrounded by them, was entirely deſtroyed. Thus the incapability of the gaſtric juice to diſſolve vegetables, and its efficacy on fleſh, were fully proved.

CLXV. By means of little ſpunges I procured this fluid ſometimes when the ſtomach was empty, and at others when it contained ſome remains of the food, in which caſe it was always turbid and full of heterogeneous matters, of a cineritious yellow colour, and had not much fluidity. When the ſtomach was empty it was ſufficiently clear, without any extraneous ſubſtance, had an intermediate colour between yellow and white, was very fluid, and had a ſaltiſh and bitter taſte. With this I attempted experiments on digeſtion out of the body, like thoſe I have before mentioned in ſeveral paſ-

ſages.

ſages. The reſult was not different. I obtained the ſolution of various kinds of fleſh by renewing the liquor from time to time, and by applying a heat of thirty deg. the common temperature of theſe animals. With theſe precautions I moreover cauſed nearly the half of a ſplinter of a bone of beef, weighing forty-four grains, to be diſſolved.

CLXVI. Having made theſe experiments, in my opinion the moſt intereſting the ſubject admits, my next buſineſs was to examine the ſtomach and œſophagus. However, three hours before I killed my falcon, I fed him, in order to ſee what effect is produced upon the food in the craw. It was in part in this cavity, and part had deſcended into the ſtomach, where it had begun to be decompoſed. It was immerſed in the gaſtric fluid, and this incipient digeſtion had the ſame appearance as it has out of the body. The fleſh in the craw, even that which was upon the point of paſſing into the ſtomach, was only a little diſcoloured; this circumſtance ſhews that digeſtion is performed only in the latter cavity, and that in the craw the food is only diſpoſed to be diſſolved more readily.

CLXVII. When a ligature is made below the pylorus, and air blown in at the top of the œſophagus, this part of the alimentary canal reſembles a large inteſtine about five inches long; a little more than half way down the œſophagus is dilated and forms the craw, though we ſhall find, that it has this name improperly, if we compare it with the craws of gallinaceous fowls, which lie at the ſide of the œſophagus, or rather without it; whereas in the falcon the craw is a continuation of that cavity. If we invert and again inflate the œſophagus, and then examine it in a ſtrong light, or with the microſcope, we can perceive an immenſe number of glands from the beginning to the fleſhy faſcia, not excepting the craw. If we blow in freſh air, and obſerve it again with the glaſs, we ſhall ſee the glands, which are of an oblong ſhape, and project a little above the plane of the œſophagus, each emit a drop of liquid; this liquid is ſo viſcid, that one of theſe drops may be drawn out into a filament an inch or more long; it is inſipid to the taſte. The moſt conſiderable part of the œſophagus, is full of theſe glands, and is entirely membranous; it only becomes muſcular at the commencement of the faſcia, which in the falcon, as well as other birds, ſeems to conſiſt only of numberleſs follicular glands, and is above an inch in breadth. Theſe follicles are cylindrical, and are all connected by a fine membrane; they have one of their extremities implanted in the external, and the other in the nervous coat of the ſtomach: through the latter, the excretory ducts open and diſcharge the ſame kind of whitiſh and viſcid matter that has ſeveral times been deſcribed as belonging to birds that have ſimilar follicles. Theſe glands and follicles abundantly ſupply the ſtomach with fluid; and though it is ſometimes deſtitute of glandular bodies, yet a liquor continually poured into the cavity by exhalant arteries, forms an addition to that which comes from the œſophagus, as is evident from the moiſture which appears upon the ſides when they have been wiped dry ſeveral times.

CLXVIII. The eagle on which my experiments have been made, belongs to the ſpecies called by Mr. Buffon the *common eagle*, becauſe it is found upon moſt of the high mountains of Europe; it was known to Ariſtotle, by whom it is called Μελαινάετος, or the black eagle. Hence it has received the denomination of *Falco Melanpetus* from Linnæus, who refers, with whatever propriety, the eagle and falcon to one family. Though ſome naturaliſts reckon two ſpecies of the common eagle, the brown and the black, I ſhould incline with Buffon and Ariſtotle to ſuppoſe, that there is only one. The difference in colour may depend on the difference of age; for we often ſee animals of different ages belonging to the ſame ſpecies differ in colour. At the time I was in poſſeſſion of my eagle I had an opportunity of ſeeing five others, four dead and prepared, and one living, in the poſſeſſion of the counts Caſtiglioni of Milan, two noblemen equally remarkable for politeneſs of manners and ſkill in natural philoſophy. Theſe animals all varied from each other in colour, ſome being of a black more or leſs deep, and others of a darker or lighter brown; yet they agreed in the eſſential characters of the ſpecies. They were all nearly of the ſame ſize, ſomewhat exceeding that of a turkey-cock, their legs and feet were covered with feathers, the nails were black, the feet yellow, the bill blueiſh, and the baſe was covered with a bright yellow cere: ſuch are the characters which, according to the French naturaliſt, the brown has in common with the black eagle.

CLXIX. The ordinary food of my eagle conſiſted of live cats and dogs, when I could procure them. It eaſily killed dogs much larger than itſelf. When I forced one of theſe animals into the apartment where I kept the eagle, it immediately ruffled the feathers on the head and neck, caſt a dreadful look at the dog, and taking a ſhort flight, immediately alighted on his back. It held the neck firm with one foot, by which the dog was prevented from turning his head to bite; and with the other graſped one of the flanks, at the ſame time driving the talons into the body; and in this attitude it continued, till the dog expired, in the midſt of fruitleſs outcries and efforts. The beak had been hitherto unemployed, but it was now uſed for making a ſmall hole in the ſkin, which was gradually enlarged; from this the bird began to tear away and devour the fleſh, and went on till it was ſatisfied. I muſt not omit obſerving, that it never ate any ſkin, or inteſtine, or bone, except very ſmall ones, ſuch as the ribs of cats and ſmall dogs. Notwithſtanding this ferocity, and violent impetuoſity in attacking animals, it never gave any moleſtation to man. I, who was the feeder, could ſafely enter the apartment where the bird was kept, without any means of confining its movements, and beheld theſe aſſaults without dread or apprehenſion: nor was the eagle at all hindered from attacking the living prey I offered it, or rendered ſhy by my preſence. As it was not always in my power, or at leaſt in my will, to give it living food (for I had not always dogs and cats at hand; and gallinaceous fowls, which are equally acceptable, were too expenſive) I ſubſtituted fleſh which, though it was not ſo well reliſhed, was not diſagreeable. In general, when it had fleſh at will, it only made one meal a day. I found, by weighing what it ate, that thirty ounces of fleſh ſerved it one day with one another. This ſpecies of eagle is provided with a very large craw, which of courſe is the firſt receptacle of the food; and when it was at liberty to eat its fill, this viſcus was generally diſtended to a larger ſize than that of a turkey-cock full of grain. It gradually contracts in proportion as the fleſh paſſes into the ſtomach, juſt as it happens in gallinaceous fowls.

CLXX. Some of the firſt times I obſerved my eagle eat, I was ſtruck by a phænomenon, which conſtantly recurred whenever it took food. After it had ſwallowed a few mouthfuls, a thin ſtream began to flow from each noſtril, and to run down the upper ſide of the beak; at the end they joined, and formed a large drop, which ſometimes fell on the ground, but generally paſſed into the mouth, and was mixed with the food. This drop was continually renewed by freſh ſupplies from the noſtrils, as long as the animal continued to feed, and after that it ceaſed to appear. This liquor was of a ſky-blue colour, had a ſalt taſte, and was nearly as fluid as water. But why does it flow only while the eagle is feeding? and what is its uſe? It flows at that particular time only, I ſuppoſe,

poſe, becauſe the receptacle in which it is contained is then only compreſſed; and the preſſure ariſes from the motion of the mouth, or the impulſe of the food againſt the palate, near which this receptacle lies. Of the uſe of this fluid, I candidly own my total ignorance. I ſuſpect, however, that as it is mixed with the food, it ſerves, like the ſaliva, to moiſten it, and facilitate digeſtion.

CLXXI. It is commonly thought, and the opinion has the ſanction of the beſt naturaliſts, that birds of prey, and eſpecially eagles, never drink. What I have obſerved is, that the ſpecies mentioned in the preſent diſſertation, were left even for ſeveral months without water; they did not ſeem to ſuffer the ſmalleſt inconvenience from the want of it; but when they were ſupplied with water, they not only get into the veſſel, and ſprinkle their feathers like other birds, but repeatedly dip their beak, then raiſe their head, in the manner of common fowls, and ſwallow what they have taken up; hence it is evident that they drink. For the eagle it was neceſſary to ſet the water in a large veſſel, otherwiſe, by its attempts to drink, the veſſel was ſure to be overturned.

CLXXII. To collect into one point of view every thing relative to digeſtion, let us examine another opinion, more immediately connected with our ſubject. It is ſaid by ſeveral celebrated naturaliſts and phyſiologiſts, that the eagle, when unable to procure fleſh, will feed upon bread. To aſcertain this point, I made various experiments. I firſt ſet before the bird both fleſh and wheaten bread; and finding that it ran towards the

fleſh,

fleſh, without even caſting a look upon the bread, I ſet only the latter before it, and this after a day's faſt, when it muſt have been preſſed by hunger; I did not however attain the end I had in view, and therefore kept it faſting for another day, but ſtill to no purpoſe. When the bread was ſet near it, it would juſt look at it, and then turn its eyes towards ſome other object. When I had prolonged the faſt to the fourth day, the bird ran towards me, as I opened the door of the apartment, but with no other view than to aſk for food; I offered it a piece of bread, but in vain, for, without even touching it, it returned to the place where it ſtood before my coming in. I might have carried the trial ſtill further, but was afraid of the animal ſinking under it.

CLXXIII. I therefore abandoned this mode of experiment, and thought it would be better to make the eagle ſwallow ſome bread; for it would either be always thrown up, and then it would be reaſonable to infer, that this was an unſuitable kind of food; or in caſe it ſhould neither be vomited, nor voided unaltered along with the excrements, and the animal ſhould ſhew no ſymptoms of uneaſineſs, we muſt conclude that it is digeſted and aſſimilated. I concealed the bread in ſome fleſh, as I had done in my experiments upon the falcon (CLVII), and had recourſe to the ſame expedient, whenever I was deſirous that my eagle ſhould take tubes or other ſubſtances. For though this ferocious bird was exceedingly gentle towards me, who was his feeder, yet it might have been hazardous to irritate it; and that would have been unavoidable, if I had opened the beak, and thruſt bread down the throat by force. The firſt portion of bread which the eagle ſwallowed concealed by fleſh, amounted to half an ounce. Indigeſtible bodies, ſuch as feathers, uſed to be thrown up eighteen, twenty, or, at moſt, twenty-four hours after they were received into the ſtomach. But the bread was not vomited in that period, or a day longer; nor did the excrements appear to be altered or mixed with bread. I then gave the animal a whole ounce, inſtead of half an ounce of bread, none of which was vomited or voided unchanged at the vent. The ſame thing took place, when the quantity of bread was increaſed to ſix ounces. My laſt experiment upon bread, was to ſubſtitute the cruſt inſtead of the crumb; but the reſult was juſt the ſame; and, notwithſtanding the eagle had ſhewn ſo little appetite for this kind of food, its health did not appear to ſuffer. And I was obliged to conclude, that this ſpecies of vegetable is digeſted, and converted into real nutriment, as well as animal matters. I could not therefore refuſe to accede to the opinion of thoſe, who affirm that eagles, when much preſſed by hunger, will feed upon bread, though mine would not touch it.

CLXXIV. But in what manner is bread digeſted in the ſtomach of the eagle? Is it by the gaſtric juices alone, or aſſiſted by trituration? Is any ſuch action exerted by it? In ſhort, what is the immediate cauſe of digeſtion? Theſe queſtions are too cloſely connected with the object of my enquiry, to be paſſed over unnoticed. To begin then with the firſt. Tubes employed in my uſual manner, would determine the mode of digeſtion. And in the preſent caſe, I obſerved what I had before obſerved in ſo many other animals, that trituration had no part in this function, and that it was the ſole effect of the gaſtric juices. While the eagle retained the tubes, a ſpace that never uſed to exceed twenty-four hours (CLXXIII), the bread which they contained was completely diſſolved. If they happened to remain longer in the ſtomach, the gaſtric fluid had corroded the bread, and given it a yellowiſh colour and a bitteriſh taſte. Where the action of that fluid had been chiefly exerted, the bread was changed into a gelatinous paſte, which had nothing of its original taſte.

CLXXV. But the tubes ſhewed, that the gaſtric liquor of the eagle diſſolves not only bread but cheeſe, at leaſt that ſort which in Auſtrian Lombardy goes under the name of *formaggio piacentino* or *lodigiano*. This power, poſſeſſed by a bird properly carnivorous, of digeſting a ſubſtance ſo different from fleſh, induced me to try whether it is capable of producing the ſame effect on other matters, and particularly vegetables. But with reſpect to the latter, I did not find that the efficacy of the gaſtric fluid extended any further than bread; for ſeveral ſeeds of the cerealia, both raw and boiled, did not appear to undergo any alteration in the tubes, or when looſe in the ſtomach. It is ſomewhat ſurpriſing, that this ſhould be the caſe with wheat, when wheaten bread is ſo perfectly digeſted. We ſee at leaſt, that vegetables muſt be triturated before

fore they can be digeſted by the eagle, as well as by gallinaceous fowls (XLV).

The foregoing experiments, and the concurring obſervations of others (CLXXII), ſhew, that ſome animals, ſuppoſed to be ſtrictly carnivorous becauſe they live always upon fleſh, and are provided with the moſt formidable weapons for ſeizing and deſtroying their prey, may yet, under certain circumſtances, change their diſpoſition and manners, and become frugivorous. Thus we read of animals naturally herbivorous, as horſes, ſheep, oxen, gradually quitting their uſual aliment, and learning to live upon fleſh (*a*). I too can produce a recent inſtance in a young wood-pigeon, a ſpecies of bird which is univerſally known to feed upon any thing rather than fleſh. By dint of hunger I brought it gradually to reliſh fleſh ſo well, that it refuſed every other kind of ſuſtenance, even grain, of which it is naturally ſo greedy. Such changes, whether effected by deſign or accident, will not excite the ſmalleſt degree of ſurprize in thoſe who know, that of the various kinds of food uſed by man and animals, the gelatinous part ſupplies the nutriment, and that this exiſts alike in vegetables and animals (*b*). The example of the eagle among carnivorous, and of the horſe, ox, pigeon among frugivorous animals, do not however warrant us to conclude, that the former can be univerſally converted by art or chance into the latter, and reciprocally; for, on the other hand, we

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(*a*) Haller. Phyſ. T. 6.

(*b*) Ib. T. 1.

have Reaumur's kite (CXVI) and my owls and falcon (CLXVI, CLVI, CLXIV), which were incapable of digeſting vegetable ſubſtances (*a*); not that theſe ſubſtances are unfit for affording them nouriſhment, but becauſe the gaſtric liquor is incapable of decompoſing them, and extracting the nutritious jelly.

CLXXVI. With reſpect to the ſecond queſtion, whether the ſtomach of the eagle triturates its contents? I think I have abundant proof, that it poſſeſſes no ſuch power. Not to mention the numerous tin tubes that remained ſo long in it without receiving the ſlighteſt injury, I can ſafely affirm, that I

(*a*) Mr. Batigne, in his critical reflections on the experiments of Reaumur pretends, that we are are not to conclude, becauſe vegetables undergo no change in the ſtomach of the kite, that the gaſtric liquor has no action upon them. He ſuppoſes, that its inefficacy aroſe from the vegetables not having been previouſly maſticated. *Premiere Reflexion ſur les Experiences de M. de Reaumur.*

But in this Mr. Batigne is miſtaken. After I had compleated my diſſertations on digeſtion, I procured a kite of the ſame ſpecies as that of Mr. Reaumur, and had it therefore in my power to repeat and vary his experiments. I conſtantly found, that bread, grain, &c. were thrown up unaltered, both when encloſed in tubes and looſe in the ſtomach, though they had been previouſly well maſticated. This fact agrees with my obſervation on the falcon, of which the gaſtric liquor could not digeſt maſticated crumb of bread. I will add, that an owl, fed with chewed bread alone, died upon the fourth day; and upon diſſection, the bread was found in its ſtomach undigeſted. It is therefore evident, that the incapability of the gaſtric liquor of ſome animals to digeſt vegetables does not ariſe from the want of previous trituration or maſtication, and that this fluid is eſſentially unfit for diſſolving ſuch ſubſtances.

could never perceive the ſmalleſt contuſion upon the grain (which I gave the bird naked in order to try whether it could digeſt it) (CLXXV), whether raw or boiled; in which caſe, the ſmalleſt compreſſion or impulſe would have left evident marks upon the ſurface. Theſe facts are confirmed by the following obſervation: I took ſome ſtrips, about a line in breadth and three inches in length, of exceedingly thin ſheet lead, and rolling them up in the form of a ſpiral, introduced them in ſome pieces of fleſh into the ſtomach of the eagle, in which they continued eighteen hours. The leaſt force would have ſufficed to have deſtroyed the ſhape of theſe ſtrips, and being totally inelaſtic, they would preſerve whatever alteration or diſtortion they might receive from preſſure or percuſſion. However, when thrown up they retained their ſpiral form; a clear proof that they had not been ſubjected to violence of any kind.

Let it not however be ſuppoſed, that I mean to exclude motion entirely from the ſtomach of the eagle. Having frequently found foreign ſubſtances within the tubes, and fixed in the perforations, I could not but ſuppoſe, that they had been driven into them by ſome force, and this force could be no other than the agitation of the ſtomach, which was either extrinſical, and produced by the adjacent viſcera, or the periſtaltic movement by which the food is expelled through the pylorus. I only aſſert, that the ſtomach of the eagle has no action capable of breaking and triturating the aliment, as I think I have abundantly proved. It is likewiſe clearly aſcertained, that the gaſtric fluid is the efficient cauſe of digeſtion by the experiments made with bread and cheeſe encloſed in tubes (CLXXV); but this will be more ſatisfactorily ſhewn by the experiments relative to the digeſtion of animal ſubſtances, which I am now to relate.

CLXXVII. The firſt thing I wiſhed to know was what changes fleſh undergoes in the craw, and I had therefore to contrive a method of getting it back at pleaſure. Had this bird been of the ſame gentle and peaceful diſpoſition as gallinaceous fowls, this would eaſily have been effected; for I ſhould have had only to preſs the portion of fleſh that lay higheſt in the craw upwards with my thumb and fore-finger, and by a continuance of this manœuvre ſhould have brought it out at the mouth. By this ſimple contrivance I have often examined grain from the craw of fowls, pigeons, and ſuch birds; but the ſtrength and ferocioufneſs of the eagle altered the caſe totally. After much reflection, I thought of an artifice eſſentially the ſame as that adopted for gallinaceous birds. I gave my eagle only three or four pieces of fleſh, of which the laſt was tied in the ſhape of a croſs with a fine packthread three or four feet long. The eagle, preſſed by hunger, devoured the fleſh greedily without regarding the ſtring, of which the greater part hung out of the mouth; nor did the bird make any efforts to ſwallow or throw it up. When I thought it time to examine the piece of fleſh I pulled the ſtring forcibly, and the eagle, without growing enraged, opened its beak and allowed me more room for recovering the ſtring, and by conſequence the fleſh that was faſtened to it. Sometimes I uſed conſiderable force, but did not ſucceed, probably on account of the fleſh being got too low down in the craw; in this caſe, to free the eagle from the inconvenience, I cut the ſtring cloſe to the beak, and gave it ſome fleſh, which carried down the pack-thread before it into the ſtomach, whence it was thrown up in a ſhort time; but I have more frequently ſucceeded in drawing up the fleſh, and thus obtained an opportunity of examining it at leiſure. I never could find, that the craw or its juices are capable of digeſtion. Its weight was nearly the ſame after it was drawn up as before it was ſwallowed, nor did it ſeem as if it was upon the point of being digeſted; the ſurface was only a little tenderer, and had loſt its redneſs; it was imbibed with a fluid that was neither ſalt nor bitter, but quite inſipid. Fleſh therefore is not digeſted, it is only macerated in the craw of the eagle, as grain and graſs in the craw of gallinaceous fowls.

CLXXVIII. We muſt therefore conclude, that the whole proceſs of digeſtion begins and ends in the ſtomach. If then it was of conſequence to know what happens to fleſh in the craw, it is of much greater importance to obſerve how it is altered in the ſtomach. But as the expedient to get the fleſh back, mentioned in the laſt paragraph, would not be of any ſervice, I contrived another, which anſwered wonderfully well. I encloſed the fleſh I gave my eagle in little nets with ſmall meſhes, which were generally vomited empty; but in ſome there were conſiderable remains of fleſh. The pieces I uſed for theſe experiments were globular, and the remains almoſt always retained that figure. They were thoroughly impregnated with gaſtric liquor, and had both a bitter and ſalt taſte. The ſurface was gelatinous; when this was removed, the fibres were eaſily diſtinguiſhable, but were as tender as if they had been boiled, and the colour was changed to a reddiſh blue. When this ſtratum of tender fibres was taken off with a ſharp knife, that below was firmer and leſs diſcoloured, and at the center the fleſh did not appear to have undergone any change either in its conſiſtence or colour. It is needleſs to obſerve that theſe experiments prove, that the gaſtric fluid diſſolves fleſh. The permanency of the globular form clearly ſhews, that trituration does not take place, but the whole effect, to repeat it once more, is produced by the gaſtric liquor, which acts upon the ſurface, and diſſolves one ſtratum after another till the whole is conſumed, as we have ſeen the ſame liquor of other animals act as well upon fleſh as other ſubſtances (LXV, CI).

CLXXIX. This laſt experiment rendered it ſuperfluous to try, whether the gaſtric fluid of the eagle will diſſolve fleſh encloſed in tubes. Taking this for granted, I proceeded to enquire, whether digeſtion would be retarded in proportion to the toughneſs of the fleſh with which they were filled. With this view ſome of the liver, of the muſcular fleſh of the thigh and heart, a bit of the brain, and a piece of tendon were encloſed in ſo many diſtinct tubes. They continued thirteen hours in the ſtomach, and the gaſtric fluid acted upon them juſt as I had imagined it would. The tube containing the piece of brain was quite empty; of the liver only a very ſmall part remained; the reſiduum of the muſcular fleſh of the thigh was more conſiderable; that of the heart was ſtill greater; but of the tendon there remained moſt of all. Theſe remains of fleſh and tendon had the ſame appearances as I had obſerved in the balls of fleſh that were introduced into the ſtomach without tubes. The gelatinous matter on the ſurface, the tenderneſs of the fibres lying immediately below, and the conſiſtence of thoſe at the center clearly ſhewed, that the gaſtric juices had acted upon the fleſh encloſed in tubes juſt as upon what was left looſe in the ſtomach (CLXXVIII).

CLXXX. My next wiſh was to know whether its activity would be impaired or deſtroyed by paſſing through linen before it got to the fleſh. With this view, two pieces of the ſame tendon and heart, equal in ſize to thoſe employed in the foregoing experiment, were put into two linen bags, and given to the eagle; in eighteen hours they were thrown up. At firſt the ſides of the bags were diſtended by their contents, but now that which contained the fleſh was a good deal collapſed; for half of it was diſſolved: the other had more of its original diſtenſion; for not above one-third of the tendon was conſumed. Upon comparing together the diminution of the ſubſtances in the bags and in the tubes (CLXXIX), I found, that in the former caſe it was leſs, notwithſtanding the bags continued eighteen hours, and the tubes only thirteen. It is therefore evident, that the linen is a greater obſtacle to the action of the gaſtric liquor than the tubes.

CLXXXI. From my experiments upon crows (LXVII) it was obvious to conjecture, that as more folds of linen were wrapped round the animal ſubſtances, the action of the gaſtric liquor would be ſtill leſs conſiderable. I therefore gave the eagle ſix bags, containing each an equal portion of beef; the firſt was ſingle, the ſecond double, and ſo on. The bird retained them twenty-three hours, when they were all vomited at once, as uſually happened to tubes and other indigeſtible matters, which when ſmall are thrown up all at once, and when large one immediately after another. The two firſt bags were empty, and the remainder of fleſh in the four others were larger as the folds were more numerous, ſo that in the ſixth it was the largeſt of all. It had however undergone ſome diminution, and the gaſtric fluid had therefore begun to diſſolve it, notwithſtanding the ſix folds, as appeared from its being impregnated with it, and from the tenderneſs of the fibres, and the change of colour on the ſurface. My next wiſh was to try whether the juices of the ſtomach were capable of penetrating through a denſer ſubſtance; I therefore ſubſtituted cloth in the ſtead of linen, and having put ſixty-eight grains of beef in the bag, tied ſome packthread very tight round the neck. In fourteen hours it was vomited, and being apparently of the ſame ſize as at firſt, it was returned immediately into the ſtomach, where it continued twenty-two hours longer. I now found that the cloth, notwithſtanding its cloſe texture and great thickneſs which amounted to four-fifths of a line, was thoroughly penetrated by the gaſtric liquor. The fleſh was alſo moiſt with it, and appeared, upon being weighed, to have loſt twenty-ſeven grains. Twenty-ſeven grains had then been diſſolved, and as no veſtige of them was to be ſeen in the inſide of the bag, it was evident that they muſt have paſſed out through the pores of the cloth, and conſequently that the gaſtric fluid is capable of reducing fleſh to particles of the utmoſt tenuity.

CLXXXII. I have before obſerved, that the eagle devours the ſmaller bones of dogs and cats along with the fleſh (CLIX). When I gave that in my poſſeſſion a bird, it would alſo ſwallow all the bones, except thoſe of the extremities; and as they were not thrown up, there was good reaſon for believing that they were digeſted, a circumſtance that exactly agrees with my obſervations on falcons and various other birds (XCVIII, CXLVII, CLIV, CLVIII). But greater certainty was deſireable, and this I endeavoured to attain in the following manner: two pieces of the rib of a ſmall dog, each about two inches long, were tied together, and two thigh bones of a cock; this packet was retained twenty-three hours; but the bones were very much altered during that time. The two pieces of rib were reduced to the thinneſs of a membrane; the leaſt violence was ſufficient to break them; they were totally inelaſtic, and had loſt all their marrow. The two thigh bones now reſembled tubes of parchment; they were eaſily compreſſible, and when left to themſelves recovered their ſhape, and after being bent they would become ſtrait again. Upon one of the tibiæ thus waſted and altered there was a very ſingular appearance; about one-fifth was ſtill oſſeous, but tender, yielding to the touch, and much attenuated. It is therefore apparent, that the juices of the ſtomach are capable of diſſolving bone, and that in a ſhort ſpace. I was unwilling to throw aſide theſe bones thus reduced almoſt to nothing, and therefore tying them up in a bundle I gave them again to the eagle, in order to ſee whether they would be entirely diſſolved, or, like a caput mortuum, retain their membranous appearance; but being apprehenſive that this could not be ſo well aſcertained if they were naked in the ſtomach, I encloſed them in a tube. It was retained thirteen hours, and upon examination was entirely empty; it was therefore reaſonable to infer, that the gaſtric fluid had now completed the ſolution.

CLXXXIII. The readineſs with which theſe bones, of a texture by no means tender, were digeſted, led me to ſuppoſe, that the hardeſt would not reſiſt the action of the gaſtric liquor. To determine this, I began by giving the eagle a ſphere of bone worked at the lathe out of an ox's thigh bone, of the ſame diameter as that which had been uſed for the falcon, and taken from the ſame individual (CLIX). Upon that occaſion I obſerved, that the falcon did not diſſolve it during the long ſpace of thirty-five days and ſeven hours. In the preſent caſe it was every day vomited, and immediately returned, and in twenty-five days and nine hours it was completely digeſted. The eagle is then capable not only of digeſting the hardeſt bones, but of digeſting them in a ſhorter ſpace than ſome other birds of prey. In the account of my experiments on the falcon I remarked two things, firſt, that its diameter decreaſed without any change of ſhape; ſecondly, that the texture was not ſoftened during the whole time (CLIX). The firſt phænomenon occured on this occaſion, the ſphere not only maintained its figure, but continued as ſmooth as when it came from the lathe. But with reſpect to the ſecond circumſtance, there was a wide difference; for notwithſtanding the hardneſs of the bone, the ſurface was ſo ſoft every time it was thrown up, that it was eaſy to pare off ſlices with a knife, which were as pliant as cartilage. The gaſtric fluid then of the eagle, beſides diſſolving the ſuperficial ſtrata, penetrated into the ſubſtance of the bone and ſoftened it; an effect which that of the falcon is incapable of producing. Penetrating, however, as it is, it has no action on the enamel of the teeth, any more than that of the falcon (CLXI).

CLXXXIV. We have ſeen how much more ſpeedily the gaſtric fluid of the eagle digeſts bone than that of the falcon; the ſame obſervation may alſo be extended to fleſh. The former bird required thirty ounces a day (CLXIX), the latter was ſatisfied with twelve, and ſometimes with ten. The gaſtric liquor of the one then diſſolves, in an equal ſpace of time, three times as much as that of the other, and conſequently the rapidity of digeſtion in one is triple of that in the other. I ſhould however, upon mature reflection, be inclined to conſider this greater rapidity as apparent, rather than real. The eagle indeed digeſts three times as much fleſh as the falcon in the ſame time, but then the gaſtric juice of the former is far more copious than that of the latter; and if we ſuppoſe it to be three times as much, a ſuppoſition very admiſſible, as we ſhall ſoon ſee, every third part will diſſolve a quantity of fleſh equal to that diſſolved by the whole gaſtric fluid of the falcon. The ſame remark is applicable to other animals. With how ſmall a quantity of fleſh is the little owl ſatisfied in compariſon with the eagle, and conſequently how inconſiderable is the ſolution effected by the gaſtric liquor; but then how trifling does the quantity of that liquor appear when we conſider that of the eagle! The ſame reflection will recur when we compare a lamb with an ox, or a hare with a horſe. But with reſpect to the caſe in queſtion, I could not deviſe any more effectual means of determining whether the greater effect produced by the juices of the eagle aroſe from the greater abundance ſolely, or in part alſo from its ſuperior efficacy, than to give each of theſe birds a ſmall quantity of fleſh at the ſame time, and obſerve what would be the event. It would either be digeſted by one as ſoon as by the other, and then the ſame efficacy muſt be aſcribed to both; or elſe the eagle would digeſt it more ſpeedily than the falcon, in which caſe the ſmall quantity of fleſh would not allow us to ſuppoſe, that the fluid of the falcon could not ſo ſoon diſſolve it on account of its being in ſmaller quantity, and we muſt therefore conclude, that it is leſs capable of digeſting fleſh than that of the eagle. This experiment I have often repeated, not only upon the falcon and eagle, but upon the two ſpecies of owls alſo, and crows, and the reſult has been, that ſometimes one and ſometimes another of theſe birds has digeſted the ſmall portion of fleſh ſooneſt; nor did the eagle at all diſtinguiſh itſelf above the reſt. As the difference of time was very inconſiderable, it may be overlooked, and we may ſafely ſuppoſe, that the digeſtive power of the gaſtric fluid is nearly equal in theſe ſeveral ſpecies, and conſequently that the eagle has no pre-eminence above the reſt. It may however be objected, that with reſpect to bone, at leaſt, the prerogative of digeſtion belongs to the eagle in preference to the falcon, which takes above thirty-five days to diſſolve the ſame ſphere which the eagle diſſolves in leſs than twenty-ſix (CLIX, CLXXXIII). I can adopt this opinion without much reluctance, ſince there is no inconſiſtency in ſuppoſing, that two menſtrua may agree in the effects they produce upon one body, but differ with reſpect to another; nay, this idea is confirmed by the facility with which the gaſtric fluid of the eagle penetrates into and ſoftens bone, a quality which that of the falcon does not poſſeſs in the ſmalleſt degree (CLXXXIII, CLIX).

CLXXXV. Let us now proceed to ſhew the great abundance of gaſtric fluid in the eagle in compariſon with ſmaller birds, ſuch as the falcon, the owl, &c. To procure this fluid I was not obliged, as in other animals (LXXXI), to uſe ſmall ſpunges. The eagle ſupplied me ſpontaneouſly. Very ſoon after it was in my poſſeſſion I was aware, that along with the tubes a quantity of gaſtric fluid was thrown up, ſo that the floor was often quite wet with it. It was eaſy to deviſe a method of catching it before it fell on the ground; for the eagle rarely moved from the place where it took food, and therefore generally vomited the tubes on the ſame ſpot. Upon this I ſet a large glaſs veſſel, and thus was enabled to collect a large quantity of liquor, which generally exceeded three-fourths of an ounce a day on thoſe days when the vomiting took place, a quantity which I could not even hope to procure from all the abovementioned birds of prey taken together. What I obtained in this manner was extremely well ſuited to my purpoſe, not being adulterated with heterogeneous matters; for it was always thrown up when the ſtomach was empty, as I knew by the avidity which the animal ſhewed for freſh food at this time. Its ſmell, which I cannot deſcribe, is not diſagreeable, but very much reſembles that emitted by the gaſtric liquor of other birds of prey. If we except the colour, which in the others is yellow, but in the eagle cineritious, it exhibited the ſame qualities, whether we conſider the bitter and ſalt taſte, the turbid appearance, which is almoſt inſeparable from the one as well as the other, its fluidity, which nearly approaches that of water, its diſpoſition to evaporate, or the total want of inflammability.

CLXXXVI. The gaſtric juice of the eagle, as well as that of other animals, is capable of digeſting animal and vegetable matters out of the body. It has even produced an incipient ſolution of bone, and an almoſt complete one of cartilage; but the experiments were made in a conſiderable heat; for otherwiſe little or no ſolution took place, and the gaſtric juices of the eagle now only prevented theſe ſubſtances from becoming putrid.

Upon this fluid I made two experiments, to which I had not ſubjected that of other animals. On a very cold day in winter I expoſed a ſmall quantity in a glaſs, on a window, along with two other glaſſes containing water, in one of which was diſſolved a quantity of common ſalt ſufficient to give it a ſtronger taſte than the gaſtric fluid had. The thermometer ſet beſide the glaſſes ſtood at 5 deg. below 0 (*a*). Of the three liquors the firſt that was frozen was the common water, the next was the ſalt water, and the laſt was the gaſtric fluid. When I carried them into my apartment, where the temperature was three and an half deg. above 0, the firſt that thawed was the gaſtric fluid, next the brine, and laſtly the water. It muſt therefore be ſuppoſed, that this gaſtric juice is capable of reſiſting cold more than common water. As this cannot be attributed to its ſaline principle alone (otherwiſe it would have been ſooner frozen than the brine), it is neceſſary to admit ſome other principle capable of retarding congelation, whether ſpirituous or oily, or of whatever other nature; and the cloſe analogy ſubſiſting between the gaſtric liquor of the eagle and other animals

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(*a*) Twenty and three-fourths, Fahren.

renders it highly probable, that a like principle exiſts alſo in them.

My ſecond experiment was the following: having learned from Mr. Levret (*a*), that the juices of the ſtomach have the power of diſſolving the inflammatory cruſt of the blood, I procured ſome of it from a pleuritic patient, and immerſed it in a phial of the gaſtric fluid of the eagle. The event completely anſwered my expectation; for in two days and a half, in a temperature of 15 deg. the cruſt was entirely diſſolved, and converted into a liquid of a dark hue: this can occaſion no ſurprize; for if the gaſtric fluid can diſſolve animal ſubſtances of a far harder texture, ſuch as muſcle, cartilage, bone, out of the body, it will much more eaſily produce the ſame effect upon the inflammatory cruſt of the blood.

CLXXXVII. Here the death of the eagle, which happened ſomewhat more than five months after it had been in my poſſeſſion, put a ſtop to my experiments. I however reſolved to examine the parts that are ſituated internally, the only enquiry relative to digeſtion that could now be made. During the diſſection I found, that this individual was a female; for there were many eggs, ſome ſmaller and ſome bigger, attached to the ovaria. It was conſequently much larger and ſtronger than the male of the ſame ſpecies; for it is a conſtant obſervation, that the male in birds of prey is about a third ſmaller and weaker than the female; whereas, in

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(*a*) Art d'Accoucher.

other claſſes, the male exceeds the female in both theſe reſpects (*a*). The inteſtinal canal was full of the uſual folds and convolutions; when ſtretched out at full length, it was about fifty-nine inches long from the beginning of the duodenum to the end of the rectum. There is a double pancreas, and each portion is perfectly diſtinct and ſeparate; but the ſame obſervation has been made upon other animals. Both theſe glands are of a bluiſh fleſh-colour, of an oblong ſhape, and ſmaller towards the end. There is a difference in the ſize, one being an inch and an half in length, whereas the other is only an inch and three lines. They lie parallel, are ſituated about five inches from the pylorus, and ſtretched along beſide the duodenum, one on each ſide, and are attached by cellular ſubſtance. At about ſix inches diſtance from the pylorus an apparent cord, tinged internally with a dark azure-colour, lies upon the duodenum. If we trace it backwards, we find it gradually enlarged, and at laſt inſerted in the gall-bladder, which, in ſhape and ſize, reſembles a wood-pigeon's egg. From what has been before obſerved (LXXXIV, CXV), it is eaſy to gueſs the uſe of this cord; it is the duct through which the bile paſſes from the bladder into the duodenum. If the gall-bladder be preſſed gently the cord becomes immediately tinged with a deeper azure, and the liquor runs into the duodenum: if we open that gut, the upper part is found tinged with a greeniſh azure

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(*a*) Buffon. l. c. T. 1.

bile. Upon wiping it away, the entrance of the duct becomes viſible, and freſh bile runs into the duodenum when the preſſure is renewed. The gall-bladder lies towards the right lobe of the liver, but is not covered by it. The bile is rather denſe, and has a ſtrong bitter taſte.

CLXXXVIII. When I inſpected the ſtomach I was aſtoniſhed at its ſmall ſize, when compared with the crop. The latter cavity is capable of containing thirty-eight ounces of water, whereas the ſtomach can ſcarce hold three. We muſt therefore ſuppoſe, that the great quantity of fleſh devoured by this voracious bird paſſes ſlowly from the craw to the ſtomach, in proportion as it is digeſted and expelled into the inteſtines. Hence it is eaſy to comprehend how a ſingle meal may ſerve ſeveral days; for a large prey will be equivalent to ſeveral ſmaller ones. I cannot give a better idea of the ſhape of the ſtomach, than by comparing it to a man's leg and foot. At the point of the toes lies the pylorus, the foot reſembles the bottom of the ſtomach, and the leg the upper part. The fleſhy faſcia full of follicular glands, which in other birds, whether granivorous or carnivorous, is ſituated juſt above the ſtomach, in the eagle is contained within its cavity, and makes up the ſuperior and larger half. The internal coat of this faſcia is ſo thin and delicate, that it tears upon being ſlightly rubbed with a cloth. We come next to the nervous coat full of an infinite number of pores, out of which, when preſſure is made, iſſues a viſcid, cineritious, and inſipid liquor. Upon removing this coat theſe pores appea

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to be the excretory ducts of the follicles, of which one extremity adheres to this, and the other to the muſcular coat; next the laſt mentioned lies the external coat, which appears to be membranous. The glands are cylindrical, a line and one-fourth long; they are tied together by a number of membranous filaments. This ſhort deſcription ſhews the entire reſemblance between the faſcia of the eagle and other birds. The four coats paſs on to the inferior part of the ſtomach, and extend to the pylorus. The muſcular coat ſeemed to merit a diſtinct examination. It conſiſts of two ſtrata. That which lies next the nervous coat is formed by fleſhy faſciculi, of a lively red colour, running in a longitudinal direction. The other is of a paler red colour, and the fibres interſect thoſe of the other coat at right angles, and of courſe run tranſverſely. Notwithſtanding their nearneſs they are perfectly ſeparate from each other, like the rings of certain worms, particularly of the earth-worm, which they moreover reſemble in their bluiſh fleſh-colour. Theſe two thin ſtrata doubtleſs cauſe the various motions of the ſtomach, of which the effects have appeared in ſome of the experiments related above. This coat is one-fourth of a line in thickneſs; upon the faſcia it is thinner, and I could only find the tranſverſe ſtratum; whence it ſeems probable, that the motion of the ſtomach chiefly takes place in the lower part, which has no, at leaſt no apparent, glands; but as a thin tranſparent liquor oozes out on ſlight preſſure, as in the ſtomach of birds belonging to the ſame claſs (XCIII, CLI, CLXVII), we muſt conclude, that it abounds in ſmall arteries, which perform the office of glands.

CLXXXIX. The death of my eagle happened a few hours after it had taken food, nor could I diſcover the cauſe. Moſt of the fleſh was in the craw, and a little only had deſcended into the ſtomach. It lay at the bottom near the pylorus, but ſhewed no appearance of being digeſted, whether on account of the morbid condition of the animal, or becauſe it had but juſt fallen into the ſtomach. It was ſoftened by the gaſtric juices, that taſted very bitter, which, as well as its yellow hue, was owing to the regurgitation of the bile into the ſtomach, and theſe qualities were more apparent in the vicinity of the pylorus. The fleſh in the crop was not altered in conſiſtence or colour, except that which lay in contact with the ſides; this was a little diſcoloured and ſomewhat tenderer than at firſt, circumſtances that accord with what was ſaid at the cloſe of the CLXXVIIth paragraph.

Upon emptying, inſerting, and then inflating the craw, the ſurface was covered with a multitude of ſmall drops, which, when united by ſome flat body paſſing over them, formed a fluid as tranſparent and thin as water; it ſeemed to have a bitteriſh ſavour. Upon inſpecting the places whence the drops aroſe, they ſeemed ſo many points, which the microſcope ſhewed to be minute pores. Hence it appeared, that every part of the crop abounds with theſe perforations, which I had no heſitation in ſuppoſing to be the excretory ducts of a multitude of glands lying between the coats, as I had alſo found in the craws of other birds (XLIX, L, CLXVII). In ſearch of them I diſſected away the internal coat, which, in thickneſs and ſtrength, reſembles the nervous coat of the ſtomach, of which perhaps it is a continuation. But neither in the ſubſtance, or between it and the muſcular coat, did I find any appearance like glands. All that I could perceive, when I held the internal coat againſt the light, was the pores already mentioned, that looked like lucid points. Nor did the muſcular or the external coats, which laſt is membranous, contain any glandular body. I was therefore obliged to conclude, that the fluid oozing out in the form of numberleſs drops upon the internal ſurface of the craw is ſecreted, not by glands, but by arteries too ſmall to be conſpicuous. The reſt of the œſophagus is alſo full of theſe pores, and the ſame fluid iſſues out from them; no ſmall part of which muſt run into the cavity of the ſtomach, and contribute to the formation of the gaſtric menſtruum, which is compoſed of this and the proper fluid of the ſtomach, of the bile, and the pancreatic juice.

DISSERTATION V.

THE SUBJECT OF DIGESTION IN ANIMALS WITH MEMBRANOUS STOMACHS CONCLUDED. THE CAT. THE DOG. MAN. WHETHER DIGESTION TAKES PLACE AFTER DEATH.

CXC. THE great difficulty with which cats are forced to ſwallow tubes, and the facility with which they vomit them, hindered me from making experiments upon this irritable animal in the manner I could have wiſhed. Among, however, a vaſt number of unſucceſsful trials I have once or twice ſucceeded, and thus have been enabled to illuſtrate one chief object of my enquiries, I mean the efficient cauſe of digeſtion. I have uſed every effort to oblige this animal to ſwallow bread and fleſh, their ordinary food, encloſed in tubes, and in two individuals, one an adult, and the other a young one, have forced them into the ſtomach. Both were killed, one after having retained tubes filled with fleſh nine, and the other with bread five hours. The former were found near the pylorus. The outſide was wet with gaſtric juice; the grating at the ends was entire, as alſo were the tubes, upon which there did not appear any bruiſe or other injury. Two of the tubes were empty, the third contained a bit of the ſize of a lentil-ſeed macerated in the gaſtric fluid. The center preſerved the colour, conſiſtence, and taſte of fleſh; the ſurface was changed into a greyiſh jelly of a bitteriſh taſte.

The tubes containing bread having remained only five hours in the ſtomach, were not empty. It had been chewed before it was put into the tubes, by which it was moulded into the ſhape of cylinders ſix lines and three-fourths long. Theſe cylinders were not completely diſſolved, a portion about four lines long remaining towards the middle of the tube, which was externally gelatinous, but internally retained the characters of bread. This experiment then furniſhes an irrefragable proof, that the gaſtric fluid, as well in the cat as in other animals with membranous and intermediate ſtomachs, is the efficient cauſe of digeſtion independently of any triturating power.

CXCI. If the ſtomach be inverted and then inflated, it will be covered with humidity, though care ſhould have been taken to wipe it dry. This humidity will appear repeatedly after the ſtomach has been freed from it, a phænomenon common, as we have ſeen, to various other animals. It is not poſſible to diſcover the pores from which this fluid iſſues by the aid of a microſcope,

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nor can any glandular bodies be perceived in the coats, or the intervals between them; but when the ſtomach is held againſt the light, and examined with a glaſs of great magnifying power, a number of bright flat meſhes or eyes appear through the coats. I could not however determine the nature of them, notwithſtanding I conſidered the different parts of the ſtomach with ſome attention.

CXCII. My ſucceſs with dogs was much greater than with cats. I could make them take more tubes without being liable to the inconvenience of having them vomited. I could not however force them down the œſophagus, for this operation was attended with the ſame danger as in the falcon and eagle; whenever I attempted it, the animal uſed all its efforts to bite me. But as they would ſwallow them ſpontaneouſly, like thoſe birds, I had only to conceal them in pieces of fleſh, and throw them upon the floor of the place where the victim of my curioſity was kept. As I always took care he ſhould be hungry, he generally ran towards the fleſh, and ſwallowed it eagerly without maſtication; whereas, the cat would keep it in the mouth, and after chewing it for ſome time, throw out the tubes generally compreſſed by the action of the teeth, and ſwallow the fleſh only.

I repeated the experiment that ſucceeded with the two cats (CXC) upon a dog; the animal took ſix tubes, four full of various kinds of animal ſubſtances, as coagulated blood, lights, muſcle, and cartilage; the two others contained chewed crumb of bread. In fifteen hours the dog was killed, and the ſtomach examined; it contained only four tubes; the other two had not been voided, and I ſuppoſed they muſt have paſſed on to the inteſtines, where I found them among the excrementitious matter at the beginning of the rectum. Before I deſcribe the appearances in the tubes, let me ſay a few words of the juices with which the ſtomach abounded. As it contained nothing but the tubes, we may conſider them as pure. They were of a yellow colour, very bitter, almoſt without ſmell, not ſo fluid as water, and totally deſtitute of inflammability. Theſe juices evidently conſiſt of two ſubſtances, one very thin, the other viſcid and gelatinous, appeared from a depoſition of the latter after ſtanding a few hours. If the veſſel was ſet near the fire the clear part evaporated, and left a cruſt which was formed by the gelatinous matter.

The two tubes that had paſſed out of the ſtomach were empty, if we except ſome excrementitious matter that had got in through the meſhes of the grating. Of the other four three were empty, nor could I diſtinguiſh which had contained bread and which fleſh. The tough and compact cartilage alone filled part of the tube in which it had been put, but as far as I could judge by my eye, it was half waſted. It exhibited the ſame appearances as on a former occaſion; it was imbibed with gaſtric fluid, and had acquired the ſame taſte, at leaſt on the ſurface. It was ſo ſoft, that it ſeemed to approach more to the nature of membrane than cartilage.

CXCIII. The reſult of this experiment does not coincide with an obſervation in the *Prælectiones Academicæ* of the illuſtrious Boerhaave, commented upon by Haller. The paſſage is ſo important, that I muſt quote it as it ſtands in the original. "Receptum eſt in hominum opinione quod oſſa animalibus ſubigantur; cum Helmontianis olim ſenſit Boerhaavius; ut vero certior eſſet, curam adhibuit, ut obſervaret, quid cibis fieret in ventriculis animalium valde cibos coquentium, & experimento cognovit non ſubigi. Dedit cani devoranda inteſtina animalium, famelicus erat, affatim deglutiit, ſubegit minime & per extremum inteſtinum pendula miſere poſt ſe traxit. Dedit famelico cani oſſa butyro inuncta, reddidit furfura, neque quidquam diſſolvit niſi quod in aqua diſſolvi poteſt. Dedit carnes, reddidit fibras carnis exſuccas. Dedit ligamenta, ea poſt tridium nihil mutata egeſſit."

I reſerve till hereafter what I have to ſay on the famous problem concerning the digeſtion of bone by dogs, and now confine myſelf to that part of the experiment that relates to the inteſtine, fleſh, and ligament. I muſt candidly own, that the different reſults obtained by Boerhaave and myſelf ſurprized me. My ſurprize increaſed when I conſidered, that the ſubſtances were looſe in the ſtomach of his dog, and of courſe more liable to be attacked and diſſolved by the gaſtric liquor; whereas, the tubes muſt more or leſs impede its acceſs. Upon further reflection it occurred to me, that perhaps the dog was affected by ſome internal malady, which might alter the properties of the gaſtric fluid, as in the owl mentioned in the fourth diſſertation, of which the gaſtric fluid was rendered unfit for digeſtion by too long faſting (CLII). This reflection however was not quite ſatisfactory, and therefore to clear up the matter, I thought it would be better to repeat Boerhaave's experiment, and give a dog ſome inteſtine, in order to ſee what changes it would undergo in the alimentary canal. A middle-ſized dog accordingly eat four pieces of the colon and ileum of a ſheep, and at the ſame time took two tubes, containing each a portion of the ſame inteſtines. The tubes were voided before the time I had fixed for killing the animal, for both were found among the excrements within about eleven hours after they were ſwallowed. When the tubes were waſhed clean, I found that the pieces of inteſtine were about half digeſted. The ſolvent having acted upon both ſurfaces had reduced their thickneſs conſiderably, but what was left ſtill retained its original ſtructure. I then waſhed the excrements, and diſcovered ſeveral pieces of inteſtine, waſted indeed, as well as thoſe contained in the tubes, yet eaſily diſtinguiſhable.

CXCIV. This experiment does not exactly coincide with that of Boerhaave; it is not however totally repugnant to it, ſince the pieces of inteſtine were not completely diſſolved. My long acquaintance with the circumſtances attending digeſtion gave riſe to a conjecture, which I reſolved to ſubmit to the teſt of experiment. The digeſtion of theſe pieces of inteſtine, ſaid I to myſelf, was not complete in the ſhort ſpace of eleven hours (CXCIII); but may it not be ſo in a longer time? Is not the quantity of ſolution in ſome meaſure proportional to the quantity of time? Does not this appear from undeniable facts related in the foregoing diſſertations?

In order to verify my conjecture, I had only to contrive a method to prevent the inteſtine from paſſing ſo ſoon through the pylorus; and this, I conceived, might be done by enlarging the tubes beyond their uſual ſize. I got the laſt-mentioned dog to take three ſuch tubes filled with as many pieces of the large inteſtine of a ſheep, as amounted to half an ounce and four penny-weights. The tubes were concealed in pieces of the ſame inteſtine. The dog, which as uſual, was hungry at the time of the experiment and was ſtill kept faſting, voided ſome excrement in twenty-one hours, and upon minutely examining it, I thought I had ſome foundation for believing, that my conjecture was not fallacious; for though ſome membranous and fibrous fragments appeared among it, which could be nothing but pieces of the inteſtine in which the tubes were concealed, yet they were much more waſted, and much leſs eaſily diſtinguiſhable than in the former experiment (CXCIII), on account of their longer continuance in the body. As the proceſs of digeſtion is leſs rapid in the tubes than in the open ſtomach, I let the dog live twenty hours longer, at the end of which the three tubes had remained forty-one hours in the ſtomach. They all three lay cloſe together at the inferior orifice of the ſtomach, wrapped up in ſome bits of rag, which in all likelihood the animal had ſwallowed before the experiment, and both the tubes and rags were immerſed in gaſtric fluid. I make no mention of this juice, having found it to poſſeſs the properties deſcribed in the CXCIId paragraph. The reader is more intereſted in knowing what happened to the inteſtines contained in the tubes. Nothing could have ſucceeded better than this experiment: two of the tubes were empty, and what remained in the third did not amount to eleven grains; and thus I had the ſatisfaction of finding, as I had conjectured, that the incomplete digeſtion of pieces of inteſtine ſometimes obſerved in dogs, is no proof of the inability of the gaſtric fluid to diſſolve them; it only ſhews, that they have not been long enough ſubjected to its action. Hence the reaſon of Boerhaave's miſtake appears evident. Perceiving ſome inteſtine which he had given to a dog hanging out behind, he concluded that the animal could not digeſt ſuch ſubſtances (CXCIII); whereas from the facts juſt adduced it is obvious, that they had only not continued in the ſtomach a ſufficient length of time.

CXCV. Theſe facts alſo ſhew, that fleſh in the ſtomach of the dog loſes its fibrous ſtructure, which it retains only when it happens to be voided ſoon after it has been ſwallowed. But it might be objected by a rigid partizan of Boerhaave, that I have not ſtrictly proved, that the ſolution extends to the fibres; for they may have been gradually ſeparated from the common maſs, and paſſing out at the pores, and eſpecially through the meſhes of the lattice-work, have left the cavity of the tube empty. I therefore thought it would be proper to throw further light upon the ſubject by a deciſive experiment. If pieces of fleſh were incloſed in purſes of very thick linen, they would either be diſſolved and leave no veſtige behind them, as by other animals in like circumſtances (LXVII, CLXXX, CLXXXI); and in this caſe we muſt infer, that dogs are capable of diſſolving fleſh completely, or elſe the fibres would remain in the purſes, and then we ſhould be obliged to agree with Boerhaave, that the digeſtion of fleſh by dogs conſiſts in expreſſing the juices and converting them into chyme, while the ſolid parts remained unaltered. But along with the fleſh I ſubjected harder and more tenacious animal ſubſtances, ſuch as tendon and ligament, to the teſt of experiment. Six bags of very thick linen were given to two dogs; four contained four different ſorts of fleſh, viz. beef, veal, horſefleſh, and mutton; and the two others tendon and ligament of an ox. Each bag contained a quarter of an ounce; and it is to be obſerved, that the contents were not cut into ſmall pieces. Being apprehenſive leſt theſe bags, though of ſome bulk, would paſs through the pylorus before the time for examining them, I tied to each a bit of dry ſpunge. They continued four days in the ſtomachs of the two dogs; but fearing leſt ſo long a faſt might be hurtful to the animals, and of courſe diſturb the proceſs of digeſtion, I fed them ſeveral times, though rather ſparingly. At the expiration of the time juſt mentioned, I killed and immediately opened them. The experiment ſucceeded juſt as I could have wiſhed; for all the bags were in the cavity of the ſtomach. Suſpecting that they might have been torn by the teeth of the dogs, I took particular care to examine, but they were whole, and upon being cut open the four firſt were as empty as if they had never contained any fleſh, but of the tendon and ligament there remained about the ſize of a hazle-nut; there was no hole in either of the bags. The tendon appeared, upon being weighed, to have loſt three-fourths, and the ligament above one half. I examined with particular attention, whether this diminution of bulk and weight aroſe from the expreſſion of the juices, but the contrary was evident; for they were as pulpy and moiſt as at firſt. Hence I had every reaſon for concluding, that the gaſtric fluid had really attacked and diſſolved the ſolid parts, ſo as to enable them to paſs through the pores of the linen. The ſame thing had happened to the fleſh. The ſolution was ſtill further confirmed, by the condition of the external ſtrata of the tendon and ligament, which were become ſo tender as to be torn by the ſlighteſt violence. I was thus fully convinced of the efficacy of the gaſtric fluid of the dog in diſſolving the fibres both of fleſh, tendon, and ligament, though the proceſs is leſs rapid in the latter on account of their greater tenacity and harſhneſs. That Boerhaave's dog ſhould void the ligaments unchanged on the third day (*ea poſt triduum nihil mutata egeſſit*, CXCIII), if by this expreſſion he means that they retained the nature of ligament, and it ſeems incapable of any other interpretation, I have not the ſmalleſt ſcruple in believing, having ſeen the ſame thing in a ligament that had been four days in the ſtomach of a dog. It had indeed undergone a conſiderable diminution, a circumſtance which the celebrated Dutch phyſician would alſo have obſerved if, inſtead of judging by his eye, he had taken the precaution of weighing it before it was ſwallowed, and after it had been voided.

CXCVI. We now come to conſider whether dogs are capable of digeſting bone; a problem which, if we may rely upon the obſervations of ſeveral celebrated phyſiologiſts and phyſicians, would appear to be decided in the negative. We have already ſeen that Boerhaave's dog, after having eaten bones dipped in butter, produced no other change upon them than ſimple water would have done (CXCIII). This he alſo endeavours to confirm by the following remark: "Deinde in ſtercore canino, quod *Album Græcum* vocant, fragmenta oſſium pene non mutata reperiuntur, & fit mera oſſium raſura, quæ dentibus canis adroſit, exſuccorum & in unam maſſam reductorum." It appears from ſome notes on this paſſage and his great work (*a*), that his illuſtrious diſciple, Albert Haller, adopted the ſame opinion. Dr. Pozzi alſo aſſerts in his work quoted above (XIII), that dogs do not digeſt bone. Of the two experiments which he relates, the following appears the moſt concluſive. He gave a dog that had been faſting five days three bones which, though dry, the animal ſwallowed for the ſake of the butter with which they were anointed. One of the bones weighed

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(*a*) El. Phyſ. T. 6.

three ounces, another two, and the third one. In three days they were voided, and had only loſt ſix grains.

Such are the moſt powerful arguments adduced by phyſiologiſts againſt the vulgar opinion. This opinion however found in Reaumur an able advocate, one who eminently poſſeſſed the difficult art of making experiments with ſucceſs. Of the ſeveral productions by which he has ſignalized himſelf, none have contributed more to his reputation, than the two beautiful memoirs on digeſtion, which I have ſo often quoted with applauſe. To illuſtrate the preſent curious and intereſting ſubject he made the following experiment (*a*). Two compact cylindrical bones, each ſeven lines long and two in diameter, were given to a ſmall bitch, which was killed in twenty-ſix hours. The bones that were ſtill in the ſtomach were diminiſhed in bulk, and it appeared to him that ſeveral laminæ were taken away. They had moreover acquired the flexibility of horn, though they were at firſt rigid and inelaſtic. Hence he infers, that they had been in part diſſolved by the gaſtric fluid.

CXCVII. After having thus noticed the experiments of others, let me relate my own. In the ſtomach and inteſtines of the dog, mentioned in the CXCIId paragraph, I found ſeveral pieces of bone. They ſeemed to belong to ſome quadruped, probably a ſheep, and muſt have been eaten before the dog came into my poſſeſſion. I did not weigh them, but as far as I could judge by inſpection,

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(*a*) Mem. 2.

they amounted to above ſix ounces. Upon waſhing and then examining them attentively, I perceived certain ſcars and longitudinal furrows, of which I doubted whether they were produced by the gaſtric liquor or the teeth of the dog; beſides, many of the angles and edges were evidently blunted, at the ſight of which the idea of what happens to the hardeſt bodies in a muſcular ſtomach recurred. I moreover obſerved, that theſe places were not ſo hard as the thicker parts of the bone. But theſe phænomena only ſuggeſted doubts, which I reſolved to diſſipate by the light of experiment. To obtain this, I filled tubes with pieces of bone, and gave them to a dog. The bones were of various kinds and degrees of hardneſs: the tubes, which were two in number, were put in a bag of linen, in order to prevent the bits of bone from getting out. To allow the gaſtric juices a proper time for producing their effects, the dog was ſuffered to live for ſeven days, and during this time was fed moderately. One of the tubes, though they were rather large, had paſſed into the cœcum, and was ſurrounded by feculent matter; the other remained in the ſtomach. They were neither of them empty, but their contents, which at firſt weighed three-fourths of an ounce and eighteen grains, now amounted only to four penny-weights and ſeven grains. All the angles and edges were deſtroyed. The ſofteſt bones had ſuffered moſt. They could now be eaſily cut in the thin places with a knife. The thinneſt parts of the bone were diſſolved and had paſſed through the bag, not a veſtige of them remaining. This experiment proves two propoſitions, viz. that the digeſtive powers of the dog act upon bone as well as fleſh; though the latter, on account of its ſoftneſs, is more ſpeedily diſſolved, and that the gaſtric juices are the ſole efficient cauſe of digeſtion.

CXCVIII. This experiment was repeated three times, and the reſult was eſſentially the ſame; but there occurred two circumſtances that deſerve to be noticed. One of the dogs produced only a ſmall diminution of the bones in eight days, though it was fed plentifully, and ſeemed to be in perfect health the whole time. This ſhews, that little or no effect being produced upon the bone, which ſometimes happens, as in the caſes alledged by Boerhaave and Pozzi (CXCVI), is no proof of the inefficacy of the gaſtric fluid of this animal; it only ſhews, that the digeſtive powers are unequal; nor ought this to excite our ſurprize, as the ſame thing is obſervable in our own ſpecies.

The other remarkable circumſtance, to which I alluded, is the reverſe of the preceding. Among the bones given to one of the dogs, were two dentes inciſores from the upper jaw of a ſheep. It has been already obſerved, that the enamel of the teeth receives no injury from the gaſtric fluids that are capable of diſſolving the hardeſt bones, ſuch as thoſe of the eagle and falcon (CLXI, CXXXIII); yet the gaſtric fluid of the dog in queſtion, damaged this denſe ſubſtance. I have now before me the two teeth, which I keep as a curioſity. The enamel of one is corroded in two, and of the other in three places; the five cavities are above a line

long,

long, and penetrate to the nucleus of the bone. The roots of the teeth were almoſt entirely deſtroyed. The powerful menſtruum of the ſtomach, had made greater havock among the other bones that were encloſed along with the teeth; the excavations being wrought in a tenderer ſubſtance were more conſiderable. Upon comparing this phænomenon with the furrows mentioned in the CXCVIIth paragraph, I have no doubt but they were occaſioned by the gaſtric ſolvent. It deſerves to be remarked, that in the caſe where the enamel of the teeth was deſtroyed, the linen bag had not ſuſtained the ſmalleſt injury, though the ſolvent neceſſarily paſſed through it: nor is this to be wondered at; for we have inſtances of many gaſtric fluids that are capable of decompoſing the moſt compact animal ſubſtances, though they do not produce any effect on the ſofteſt vegetable matters (CXLVI, CLVI). This is alſo true of chemical menſtrua; the nitrous acid diſſolves the hardeſt calcareous ſtones, but leaves the moſt friable gypſum and clay untouched.

CXCIX. Though my experiments on dogs deciſively prove, that digeſtion is the effect of the gaſtric fluid alone; yet it was proper to enquire, whether the ſides of the ſtomach have any motion during digeſtion, and what that motion is? There were two ways of making this enquiry; mediately, that is, by the effects; and immediately, that is, by opening the abdomen and inſpecting the ſtomach.

With reſpect to the firſt mode, though I was certain that the ſtomach of the dog had no conſiderable motion, becauſe neither the tubes nor the bags had ſuſtained any injury; yet in order to ſee whether it has any motion at all, I gave a dog ſome thin tubes open at the ends, which were therefore liable to be compreſſed by the ſmalleſt violence. But I could not find the leaſt contuſion upon them, after they had been three days in the ſtomach. The inſpection, however, of the tubes, preſented a phænomenon which ſhewed, that the ſides of the ſtomach had not been inactive all the time. Upon opening this viſcus I found a maſs of hairs, which were of a different colour from thoſe of the dog, and could not therefore have been ſwallowed while the animal was licking itſelf. They muſt have belonged to ſome other animal that had been devoured by the dog, before it fell into my hands. Theſe hairs were not only floating in the ſtomach, but many of them had likewiſe got into the tubes; and this muſt have been effected by the action of the ſtomach.

CC. I opened five living dogs, taking care not to wound the ſtomach. This operation was performed ſoon after they had taken food; for I preſumed that the muſcular fibres, irritated by the diſtenſion, would contract more conſpicuouſly at this time. The ſtomach of the firſt was perfectly quieſcent when it was left to itſelf. But when the point of a knife was drawn over it, the parts that were touched and thoſe that were adjacent immediately contracted, and then returned to their former ſituation. Upon throwing round a ligature above the cardia and below the pylorus, and taking the ſtomach out of the body, I thought I perceived a ſlight periſtaltic motion, but it was of ſhort duration. The contraction and dilatation continued to ſucceed each other in the places that were touched with the knife, or any irritating body, for half an hour. The ſtomach of the ſecond was not only deſtitute of ſpontaneous motion, but was inſenſible to every ſtimulus. In the third ſtomach the periſtaltic motion was very conſpicuous; the contraction began juſt below the ſuperior orifice, and proceeded with a gentle undulation to the pylorus, and the dilatation regularly followed. This ſpectacle laſted for ſeven minutes. And I could reſuſcitate the motion by irritating the upper part of the ſtomach, but it continued only a little while. The periſtaltic movement did not appear on the ſtomach of the fourth dog, but irritation would excite it. And it was in this caſe confined to the ring or circular band correſponding to the place where the ſtimulus was applied. This band contracted gently, and the diameter of the ſtomach was ſenſibly diminiſhed; in a few minutes it dilated juſt as ſlowly. In the fifth ſtomach the periſtaltic motion was as apparent as in the third; it laſted ſome minutes longer, and when it had ceaſed in all the other parts of the viſcus, a band juſt above the pylorus continued theſe alternations. The contraction was ſo conſiderable, that the oppoſite ſides of the ſtomach almoſt touched each other; but all theſe motions were exceedingly ſlow, nor did the ſides of the ſtomach ever dilate or contract ſuddenly or forcibly.

CCI. At the ſame time I examined the ſtomach of ſome cats in the ſame manner. The reſult was exactly alike. A gradual movement of contraction and dilatation, beginning at the upper end and extending to the lower, was generally perceptible.

All theſe experiments, and the reader will find ſimilar ones in Haller, though made with a different view (*a*), clearly ſhew, that the motion of the ſtomach of the dog and cat are not capable of triturating the food, but calculated to carry it ſlowly from the ſuperior to the inferior orifice of the ſtomach, and thence expel it into the duodenum.

From the great number of dogs that were ſubjected to theſe experiments I collected a large quantity of gaſtric fluid, and found it as capable of producing an incipient digeſtion out of the body, as that of ſeveral other animals mentioned above, both of boiled and raw meat, and likewiſe of ſeveral vegetables. It was however neceſſary to apply a pretty ſtrong heat, and to change the liquor ſeveral times, as in other inſtances.

CCII. Blaſius, in his laborious and accurate anatomy of the dog, ſays, that the internal coat of the ſtomach is compoſed of a congeries of glands (*b*). My opportunities of aſcertaining this have been frequent. I have examined it with my naked eye and with the microſcope, but could never perceive any glandular appearance. Upon wiping it dry and preſſing it, it is covered with an aqueous exſudation, but I could not diſtinguiſh the

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(*a*) Mem. ſur les part. irrit. & ſenſib. T. 1.
(*b*) Anat. Anim.

pores from which this exſudation iſſues. I have examined ſeveral pieces with the ſolar and the ſimple microſcope, and in ſome perceived a vaſt number of lucid points, while in others there appeared nothing of this kind. I then examined the back part, which is contiguous to the nervous coat, and immediately ſaw, that it is compoſed of a congeries of oblong particles, of a pale fleſh-colour, cloſely compacted together. Theſe are probably the glands of Blaſius; but I cannot affirm that they are really glands, not having been able to diſtinguiſh the characteriſtic marks of glandular bodies in them. But however this may be, it is certain they are deſtined to tranſmit a fluid into the ſtomach; for whenever they are preſſed, the above-mentioned exſudation appears upon the internal ſurface. And this fluid may be expreſſed ſeveral days after the ſtomach has been taken out of the body.

I have before ſaid, that the pores from which the gaſtric liquor iſſues are inviſible; but the parts contiguous to the pylorus muſt be excepted, in which they are very conſpicuous. Upon comparing the fluid that thus oozes out with that which is collected in the ſtomach when it is opened, we ſhall find a very ſtriking difference. The latter, as we have ſeen above, is yellow, bitter, and ſomewhat gelatinous (CXCII). But the former has not one of theſe properties, being colourleſs, inſipid, and very fluid. Hence it is evident, that the gaſtric liquor of the dog, that liquor which is the efficient cauſe of digeſtion, conſiſts, as in other animals, of ſeveral different principles, viz. of ſaliva, of the œſophageal juice, of that which is peculiar to the ſtomach, of the pancreatic juice, and of bile.

CCIII. To complete my reſearches on animals with membranous ſtomachs, it remained to examine that of Man. One may indeed draw very plauſible inferences concerning human digeſtion, from obſervations on this numerous claſs; eſpecially from birds of prey, the cat and dog, which reſemble us ſo much in the ſtructure of the ſtomach. But analogical arguments are probable indeed, but not concluſive. And it is an object of much higher importance to attain certainty in Man than in animals. In the writings of antient and modern phyſicians no topic is more frequently diſcuſſed, yet there is little elſe beſides ſuppoſition: direct experiments made upon Man are entirely wanting, and their reſearches are illuminated only by the twilight of conjecture, and ſupported by precarious hypotheſes. If therefore it was neceſſary on other occaſions to have recourſe to experiment, on the preſent it was abſolutely indiſpenſible. Upon reflection it appeared, that the principal experiments were reducible to two heads, viz. to procure human gaſtric fluid, in order to examine it in the manner that of animals has been examined; and to ſwallow tubes full of various vegetable and animal ſubſtances, in order to ſee what changes they undergo in the ſtomach. I will candidly own, that the latter kind gave me ſome apprehenſion. The hiſtories of indigeſtible ſubſtances occaſioning troubleſome ſymptoms, and being vomited after a conſiderable time (*a*), occurred to my mind. I alſo recollected inſtances where ſuch bodies had ſtopped in the alimentary canal. Other facts however where the reſult was contrary, and of more frequent occurrence, gave me confidence. Thus we every day ſee the ſtones of cherries, medlars, plums, &c. ſwallowed and voided with impunity. This conſideration at laſt determined me to make a trial with as great caution as poſſible.

CCIV. I ſwallowed in the morning faſting a linen bag, containing fifty-two grains of maſticated bread. All the following experiments were made under the like circumſtances. I retained the purſe twenty-three hours without experiencing the ſmalleſt inconvenience, and then voided it quite empty. The ſtring uſed for ſewing and tying it was entire, nor was there any rent in the bag itſelf. Hence it is plain, that it had not received any damage either in my ſtomach or inteſtines. The fortunate reſult of this experiment gave me great encouragement to undertake others. I immediately repeated it with two of the ſame bags, with this variation, that one was double, and the other had three folds. My motive obviouſly was to ſee, whether theſe additional folds would impede digeſtion. The bags were voided in twenty-ſeven hours, and the double one was empty; but the other ſtill contained a ſmall quantity that had yet the characters of bread.

CCV. From vegetable I proceeded to animal ſubſtances. In a ſingle linen bag ſixty grains of boiled pigeon were encloſed, and in

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(*a*) Haller, Phyſ. T. 6.

in another the ſame quantity of boiled veal; both were previouſly maſticated. The purſes were voided in eighteen hours and three-quarters, and the fleſh was entirely conſumed. Inſtead of ſixty I next employed eighty grains, of which the bulk was not ſo great as to make me apprehend any danger from its paſsing down the œſophagus, and ſtill leſs from its getting out through the pylorus, as at this time it muſt of neceſſity be very much diminiſhed in bulk. The fleſh had been previouſly boiled and maſticated. I retained it twenty-nine hours, at the expiration of which time there remained eleven grains undiſſolved. This fleſh differed in appearance from that which is taken undigeſted out of the ſtomachs of animals. The ſurface of the latter is gelatinous, but the former was as void of ſucculency as if it had been ſet under a preſs. This appearance, which is analogus to that of the bread in the preceding experiment (CCIV), made me ſuſpect, that perhaps the human ſtomach might poſſeſs a power of compreſſing its contents, though others of the ſame ſtructure are deſtitute of ſuch a power. I therefore determined to bring this ſuſpicion to the teſt of experiment.

CCVI. Finding that I could digeſt dreſſed meat that had been maſticated, I wiſhed to know whether I was capable of digeſting it without maſtication. I ſwallowed eighty grains of the breaſt of a capon, encloſed in a bag. The bag was retained thirty-ſeven hours. So long a ſpace had produced conſiderable effects, for it had loſt fifty-ſix grains. The ſurface of the remainder was dry, but the internal fibres appeared to be more ſuc-

culent.

culent. The piece ſeemed to have been digeſted equally, for it retained its original ſhape.

CCVII. I next wiſhed to know whether this dryneſs of the ſurface would take place in raw as well as dreſſed fleſh. I did not doubt but I ſhould digeſt it in this ſtate more or leſs ſpeedily; for the human ſtomach is adapted to the digeſtion of the one as well as the other, whole nations living upon raw fleſh, and raw fiſh being eaten in ſome maritime countries; not to mention that oyſters, cockles, &c. in the ſtate they are taken, are among the delicacies of the elegant and luxurious, though a food of difficult digeſtion. I took faſting fifty-ſix grains of raw veal and as much beef, encloſed in two bags, which were returned about the middle of the next day. Of the veal, as it was the tenderer, there remained fourteen grains, and of the beef twenty-three. In both there was the ſame dryneſs on the ſurface as if the bags had been wrung, or preſſed by ſome external violence.

CCVIII. As then this phænomenon is conſtant, are we to ſuppoſe, that the digeſtion of fleſh and bread, which is produced by the gaſtric fluid within the bags, is aided by the triturating power of the ſtomach? Does any ſuch power exiſt at all? I could deviſe no better means of diſpelling theſe doubts, than by obſerving what happens to animal and vegetable ſubſtances encloſed in tubes. Should they either not be at all or imperfectly digeſted, we muſt infer, that there was wanting ſome circumſtance either neceſſary, or at leaſt expedient; and we might preſume, that it is trituration. I was then under the neceſſity of ſwallowing tubes. Having ſuffered nothing from the former experiments, I entered upon theſe without much apprehenſion. Inſtead of tin I had my tubes made of wood, fearing leſt the reſidence of the metal in the ſtomach and bowels ſhould be productive of bad conſequences, although I never perceived any in other animals. The gaſtric fluid had never corroded it, the colour was only turned to black. My wooden tubes were five lines in length and three in diameter. The ſides were, as uſual, perforated with a great number of holes, in order to allow free ingreſs to the juices of the ſtomach, along the whole length of the tubes, as well as at the ends. To prevent the entrance of the fœculent matter during their paſſage through the long track of the inteſtines, they were encloſed in linen bags, a precaution not always employed upon other occaſions of the like nature. At firſt I took a ſingle tube, containing thirty-ſix grains of boiled veal previouſly maſticated. The tube was voided empty in twenty-two hours. The cover of linen was entire, and had prevented any extraneous matter from getting in.

CCIX. This experiment, which is by no means favourable to the doctrine of trituration, induced me to attempt others before I drew any concluſion. As the tube was capable of containing above thirty-ſix grains, I put in forty-five. I retained it ſeventeen hours. There was a reſiduum of twenty-one grains; and now appearances were changed; the veal not only had its natural ſucculence, but the ſurface was ſoft and gelatinous, the center alone remaining fibrous. The jelly was ſweet, its ſmell was not at all putrid, any more than that of the reſiduums in the purſes. Theſe appearances were obſerved in three other experiments with boiled, and one with raw fleſh of ſeveral different kinds. I heſitated not to conclude, that in Man, as well as numberleſs other animals, the gaſtric fluid digeſts the food without the concurrence of trituration. It is indeed not poſſible that it ſhould concur; for I have direct proofs, that no muſcular action capable of producing ſuch effects is ever exerted by the human ſtomach. Among the wooden tubes employed in theſe experiments, I procured ſome to be made ſo thin that the ſlighteſt preſſure would cruſh them to pieces; and though I frequently uſed them, not one was ever broken. If I took off the linen cover, which was always entire, and examined them with ever ſo much attention, I could never perceive the ſmalleſt fiſſure.

CCX. Theſe perfectly coincide with the following facts. Cherries and grapes are ſaid to be voided entire (*a*). I reſolved to aſcertain by my own experience the truth of theſe obſervations. I firſt ſwallowed four unripe grapes, becauſe in that ſtate they have greater firmneſs. In a day they were all voided with the ſkin whole; the colour was changed from a greyiſh white to yellow. I next made trial of ripe grapes, which, as every one knows, burſt on the ſlighteſt preſſure. Of twenty-five which I ſwallowed eighteen were voided entire, of the other ſeven the ſkins only ap-

(*a*) Haller, Phyſ. T. 6.

peared. I made the ſame experiments with many cherries, as well ripe as unripe, and by far the greater number were voided entire. Theſe experiments, together with thoſe made on the thin tubes, afford the moſt concluſive evidence, that no triturating force is exerted by the human ſtomach.

I ſhall be perhaps aſked, what is the cauſe of the dryneſs of the fibres, ſo often obſerved in fleſh encloſed in the linen bags, which would appear to have been forcibly preſſed (CCIV, CCV, CCVI, CCVII)? Upon conſidering the matter I was led to ſuppoſe, that the inteſtines are more concerned in this phænomenon than the ſtomach. While the bags remain in the ſtomach, the fleſh is converted into a gelatinous matter; for there is no reaſon for believing this happens in the tubes only, and not in the bags. But when they are protruded into the inteſtines, they muſt be ſurrounded and preſſed by the fœculent matter. Hence the jelly is ſqueezed out, and the fibres loſe their ſucculence. And this, not the action of the ſtomach, I take to be the cauſe why cherries and grapes are now and then burſt.

CCXI. Having thus eſtabliſhed this fundamental propoſition, viz. that the digeſtion of fleſh and bread is produced in my ſtomach by the gaſtric fluid independently of trituration (CCIV, CCV, CCVI, CCVII, CCVIII, CCIX, CCX), I had before me a fine field for experiments that could not fail to ſuggeſt ſome important truth. The neceſſity of maſtication is ſufficiently known. There is, perhaps, no perſon who has not ſome time or other been ſubject to indigeſtion for want of having performed this properly. In the courſe of my experiments I had ſwallowed ſome maſticated fleſh, and ſome without maſtication; but having never taken care that it ſhould be of equal ſize, I had no term of compariſon, and hence was not certain which was moſt ſpeedily digeſted. I therefore ſupplied this omiſſion in the following manner. I took two pieces from a pigeon's heart, each weighing forty-five grains, and having chewed one as much as I uſed to chew my food, encloſed them in two tubes, and ſwallowed them at the ſame time, but without attaining the end I had in view; for the tube containing the chewed fleſh was voided in twenty-five hours, and the other in thirty-ſeven, both empty. Another experiment made under the ſame circumſtances ſucceeded better, both the tubes were voided in nineteen hours, and I then ſaw how much digeſtion is promoted by maſtication. Of the maſticated fleſh there remained only four grains, whereas of the other there were eighteen left. This was confirmed by two other experiments, one made with mutton, the other with veal. The reaſon is obvious. Not to mention the ſaliva which moiſtens and prediſpoſes meat to be diſſolved, it cannot be doubted, that when it is reduced to pieces by the action of the teeth, the gaſtric fluid penetrates more readily, and by attacking it at more points, diſſolves it more ſpeedily than when it is whole. This is true of menſtrua in general, which always diſſolve bodies ſooneſt when they have been previouſly broken in pieces. This is alſo the reaſon why in other experiments, maſticated bread and dreſſed fleſh were more readily diſſolved than unchewed bread and raw fleſh. The boiling had made it tenderer, and conſequently diſpoſed it to allow ingreſs to the gaſtric fluid.

CCXII. It is an opinion common among modern phyſiologiſts, that fleſhy fibres, tendon, cartilage, and bone loſe their juices in the human ſtomach, but that the ſolid parts are not diſſolved or digeſted. With reſpect to fleſhy fibres, I muſt differ from them, having clearly proved the contrary by experiment (CCV, CCVIII, CCXI). As I could bring the other ſubſtances to the ſame teſt, I would not neglect an enquiry of ſo much importance. I at firſt took membrane encloſed in a tube without maſtication or diviſion, weighing about ſixty-five grains. The tube was voided in thirty-two hours, and preſented the following appearances: The membrane was entire, but ſeemed thinner and ſhorter. It weighed only twenty-eight grains. This diminution, however, was not a ſufficient proof of the ſolution of the ſolid parts; it might be the conſequence of the privation of the fluids. It was therefore proper to return it into the ſtomach, and wait the reſult. The membrane was voided in fifteen hours; it was ſtill in one piece, but exceedingly reduced, weighing now only five grains. This petty remainder I ſwallowed a third time; the tube was voided in twenty-two hours, and was now completely empty. I afterwards ſaw the ſame phænomena in membranes of greater thickneſs and tenacity; I once digeſted the aorta of a calf after it had been boiled. The only difference I obſerved

I obſerved was, that the compacter membranes required more time to be diſſolved.

CCXIII. I made experiments upon cartilage and tendon at the ſame time. To avoid giving the reader diſguſt by too particular a recital, I will only mention the bare reſults. The cartilage was more ſpeedily diſſolved than the tendon, the former being totally conſumed in eighty-five and the latter in ninety-ſeven hours. Both were taken from an ox, and had been previouſly boiled for half an hour.

CCXIV. Bones ſtill remained, and I ſubmitted ſome both of a hard and ſoft texture to experiment. The latter were completely diſſolved, and required about the ſame time as cartilage. But the former underwent no perceptible diminution, though it continued upwards of eighty hours in my ſtomach. I likewiſe ſwallowed a naked ball of hard beef bone three lines in diameter, and in thirty-three hours voided it undiminiſhed.

It is therefore certain, that the ſtomach is capable of digeſting not only muſcular fibres, but membrane, tendon, cartilage, and even bone itſelf, provided it is not too hard; though moſt phyſiologiſts and phyſicians have been led to adopt a contrary opinion by obſerving, that theſe ſubſtances are evacuated unaltered. But this is no proof that they are indigeſtible (for if they had made the experiment on themſelves, and weighed the ſubſtances, they would have obſerved a waſte), it only ſhews, that they are not ſo ſoon digeſted as other kinds of food, which are diſſolved in a few hours; whereas, membrane, tendon, cartilage, &c. require ſeveral days, on account of their tenacity and hardneſs.

Let no one ſuppoſe, that my ſtomach, being ſtronger than common, is capable of digeſting what that of others cannot digeſt. I own, with concern, that it is weak, as is uſual in thoſe whoſe purſuits condemn them to a ſedentary and unwholeſome way of life. My ſtomach digeſts food ſo ſlowly, that I cannot ſtudy for five or ſix hours after a ſparing dinner, and am liable to indigeſtion whenever I feed more plentifully than common.

Before I quit this ſubject let me obſerve, that though I have mentioned the gaſtric juices as the efficient cauſe of digeſtion in the experiments on myſelf, yet I mean not to exclude thoſe of the inteſtines from their ſhare. We know, that the ſmall inteſtines complete the proceſs of *chylification*, which is but begun in the ſtomach. I muſt therefore allow, that the digeſtion of animal and vegetable ſubſtances in the bags and tubes is perfected in the inteſtines. But this is not in the leaſt repugnant to the reſult of thoſe experiments that ſhew the human ſtomach to be deſtitute of any triturating force, and digeſtion to be the effect of the gaſtric fluid alone, though the fluid which is ſecreted by the ſides of the ſmall inteſtines may complete the proceſs.

CCXV. In the CCIIId paragraph I remarked, that the chief experiments on man were reducible to two heads, thoſe which relate to the natural proceſs, as it may be obſerved by means of tubes and ſuch contrivances, and thoſe which relate to artificial digeſtion, provided the gaſtric juices can be procured. Having treated the former of theſe diviſions as well as circumſtances would permit, it remains for me to make ſome enquiries relative to the ſecond. It was firſt neceſſary to deviſe a method of procuring the gaſtric fluid. The firſt idea that ſtruck me was to ſearch for it in dead bodies, but after examining ſeveral ſtomachs I was obliged to abandon this ſearch; for they were either without any fluid, or elſe what they contained was ſo turbid and ſo much adulterated with heterogeneous matters, that it would by no means ſuit my purpoſe. Nor were the little ſpunges, which had ſerved ſo well in animals, better adapted to the preſent occaſion. Two ſpunges would not ſupply me with a ſufficient quantity, and I could venture only to ſwallow two tubes at once, for fear of forming an obſtruction in my ſtomach. Beſides, the juice thus procured would have been very impure, on account of the heterogeneous matters that the tubes muſt neceſſarily have imbibed during their paſſage through the inteſtines.

There remained only to obtain it by exciting vomiting while the ſtomach was empty. To effect this, I choſe rather to tickle the fauces than drink warm water, as in this caſe the gaſtric fluid muſt have been diluted. In this manner therefore, before I took meat or drink, I procured in two mornings a quantity ſufficient for a few experiments, of which the reſult ſhall be related below. I could have wiſhed to have made a greater number, but the diſagreeable feelings occaſioned by the act of vomiting, the convulſions of my whole frame, and more eſpecially of my ſtomach, that continued for ſeveral hours after it, left upon my mind ſuch a repugnance for the operation, that I was abſolutely incapable of repeating it, notwithſtanding my earneſt deſire of procuring more gaſtric liquor.

CCXVI. I was therefore obliged to content myſelf with what theſe two vomits afforded me. The firſt time it amounted to an ounce and thirty-two grains. It was frothy at its being thrown up, and ſomewhat glutinous. After it had been at reſt a few hours and depoſited a ſmall ſediment, it was as limpid as water. It was a little ſalt to the taſte, but not at all bitter. It did not either, when thrown on the fire or brought near a candle, ſhew any token of inflammability (*a*). It

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(*a*) From this and the LXXXIſt, CXXIIId, CXLIXth, and CLXXXVth paragraphs we may collect, that the gaſtric juices both of man and animals are deſtitute of inflammability. I made theſe experiments, becauſe Reaumur thought that that of his kite was inflammable, which quality Dr. Batigne imputes to the bile, a fluid conſiſting principally of oil (*premiere Reflexion ſur les Exper. de Reaumur*). But were this true, the gaſtric juices of moſt of my birds ought to have taken fire. As all mine are contrary to Reaumur's ſingle experiment, I ſhould ſuſpect, that what he obſerved was owing to accident. His experiment was the following: To take away the ſmell of putrid fleſh, which one of his tubes had acquired, he ſet it upon ſome burning coals, when immediately there iſſued a flame from the inſide that laſted above a minute (Seconde Mem.). But it is eaſy to perceive, that this might have been owing to ſome fat of the fleſh encloſed adhering to the tube. I am more confirmed in this ſuſpicion from having obſerved, that the gaſtric fluid of a kite, ſuch as Reaumur's, mentioned in a note to paragraph CLXXV. was not more diſpoſed to take fire than the other gaſtric juices which I examined.

evaporated in the open air, and when I put fifty-two grains into a veſſel and ſet it on hot coals, it emitted a thick ſmoke. Another ſmall portion, weighing eighty-three grains, was put in a phial, which was cloſed with a ſtopple to prevent it from evaporating. It did not change colour or taſte, nor did it acquire any bad ſmell, notwithſtanding it was kept above a month in the hotteſt ſeaſon of the year. I thus employed about one half, the remainder was uſed for an attempt to obtain artificial digeſtion. It was put into a glaſs tube two inches long, ſealed hermetically at one end, and very narrow at the other; I then introduced a ſmall quantity of maſticated boiled beef, and ſtopping the tube with cotton, ſet it in a ſtove cloſe to a kitchen fire, where there was a conſiderable heat, though not perhaps exactly equal to the temperature of my ſtomach. By the ſide of this tube I placed another, containing the ſame quantity of fleſh immerſed in water. The appearances in both were the following: In twelve hours the fleſh in the former began to loſe its fibrous ſtructure, and in thirty-five it had ſo far loſt its conſiſtence, that when I attempted to lay hold of it, it ſlipped from between my fingers. But though to the naked eye it appeared to be reduced to a pultaceous maſs and to have loſt its fibrous texture, yet the microſcope rendered the fibres viſible; they were however reduced to a great degree of minuteneſs. After this ſemifluid ſhapeleſs maſs had continued two days longer in the gaſtric fluid, the ſolution did not ſeem to have made any further progreſs, and the reduced fibres were ſtill juſt as apparent. The fleſh did not emit the leaſt bad ſmell, while that immerſed in water was putrid in ſixteen hours, and became worſe and worſe the two following days. It loſt in ſome meaſure its fibrous ſtructure, as always happens during putrefaction; but this appearance did not proceed ſo far as in the other portion, for the fibres were entire on the third day.

CCXVII. I vomited the ſecond time more gaſtric fluid, and was now enabled to examine it again as I had done before; and it appeared to poſſeſs exactly the ſame properties. In order to determine the influence of heat two tubes were filled with it, and ſome fleſh was immerſed as before (CCXVI). One of the tubes was placed in the ſtove, and the other left in the open air. In the former the fleſh was juſt as much diſſolved as in the preceding experiment; but in the latter the ſolution proceeded no farther than when water was employed (CCXVI). There was however no putrid ſmell, though the fleſh was left immerſed in the gaſtric fluid ſeven days.

Before I conclude this account, I muſt mention a circumſtance that happened the ſecond time I procured gaſtric liquor by vomiting. Four hours before I ſubmitted to this diſagreeable operation, I had ſwallowed two tubes filled with beef, one of which was thrown up; the fleſh was throroughly ſoaked in the fluid of the ſtomach, and the ſurface was ſoft and gelatinous; it it had moreover waſted from fifty-three to thirty-eight grains. This experiment proves, that there is a conſiderable degree of digeſtion in the

ſtomach,

ſtomach, before the food paſſes into the inteſtines.

CCXVIII. We may now ſafely lay down ſome general conſequences concerning digeſtion in Man and animals. In the experiments on birds with muſcular ſtomachs, we have ſeen how trituration diſpoſes the food to be digeſted. Hence Nature has furniſhed this claſs with gaſtric muſcles of ſufficient power to effect this neceſſary preparation. But we have likewiſe ſeen how digeſtion, which conſiſts in the tranſmutation of the aliment into chyme, is the effect of the juices alone with which the ſtomach abounds (Diſſ. I).

We next proceeded to birds with intermediate ſtomachs, ſuch as crows and herons, and found, that in them digeſtion was owing to the gaſtric fluid alone (Diſſ. II).

We next conſidered animals with membranous ſtomachs, a claſs ſo numerous and various, that it comprehends almoſt every family of living creatures; it includes the inhabitants of ſalt and freſh water; amphibious animals, as the frog, the newt, and water-ſnake; reptiles, as the viper, the land-ſnake, and many others; quadrupeds, as the cat, the dog, the horſe, the ox; birds, as birds of prey: to this catalogue Man himſelf is alſo to be added.

In ſeveral of theſe animals we have ſeen the neceſſity of previous trituration, as in the ruminating order and in Man; in them it is produced by the teeth, as in gallinaceous fowls by the muſcles of the ſtomach. But in others, as in the frog, the newt, ſerpents, and birds of prey, it does not at all contribute to digeſtion. But in the latter, as well as the former caſes, we have ſeen how the food is diſſolved and digeſted by the gaſtric fluid (Diſſ. III, IV).

In every order of animals, Nature, ever uniform in her operations, employs one principle for the performance of this vital function. Hence ſhe has ſo copiouſly furniſhed the œſophagus and ſtomach with glands, follicles, and other contrivances that anſwer the ſame end, whence continually flow the juices ſo neceſſary to the life of Man and animals. Theſe juices agree in many properties, but the difference of effect ſhews, that they differ in others. In the frog, the newt, ſcaly fiſhes, and other cold animals, the gaſtric fluid produces digeſtion in a temperature nearly equal to that of the atmoſphere. But the gaſtric fluid of hot animals is incapable of diſſolving the aliment in a degree of heat lower than that of the animals themſelves. There is alſo a difference in celerity of action, and in efficacy. In celerity, becauſe the food in hot animals is digeſted in a few hours; whereas, in the oppoſite kind it requires ſeveral days and even weeks, particularly in ſerpents. In efficacy, becauſe the gaſtric juices of ſome animals, as the gallinaceous claſs, can only diſſolve bodies of a ſoft and yielding texture, and ſuch as have been previouſly triturated; while thoſe of others, as ſerpents, the heron, birds of prey, the dog, decompoſe ſubſtances of great tenacity, as ligament and tendon, and of conſiderable hardneſs, as the moſt rigid bone. Man belongs to this diviſion; but his gaſtric fluid ſeems to have no action on the hardeſt kind of bones. Further, ſome ſpecies, as birds of prey, are incapable of digeſting vegetables. But Man, the dog, the cat, crows, &c. diſſolve the individuals of both kingdoms alike. In general theſe juices produce their effects out of the body, as the numerous inſtances of incipient digeſtion under this circumſtance, both with the gaſtric fluid of animals and Man abundantly ſhew.

CCXIX. Having thus brought into one point of view the principal circumſtances relative to the efficient cauſe of digeſtion, let us compare them with what has been moſt plauſibly written upon this topic ſo intereſting to the phyſician. The opinion that prevails chiefly in the ſchools of Europe is that advanced by Boerhaave, who has in truth done nothing but reconcile the opinions that had been propoſed at different times before him. He obſerves, in the firſt place, that the various ſolid and fluid ſubſtances which ſerve for food, being received into a cloſe, moiſt, and warm veſſel, muſt, according to the nature of each, ſooner or later begin to ferment or putrefy. There are alſo various fluids continually running into the cavity of the ſtomach, viz. the ſaliva, the œſophageal liquor, that thin tranſparent fluid which is ſecreted by the gaſtric arteries, and a viſcid humour ſecreted by glands in the ſtomach. If we conſider the properties of theſe ingredients, and moreover take into the account the remains of the food which ſerve as a ferment, the air which produces an inteſtine movement of the integrant parts, the heat which excites this heterogeneous maſs, we ſhall find, that the aliment will be macerated, diluted, attenuated, diſſolved, determined to an incipient fermentation, and in ſhort, impreſſed with the primary principle of vitality. Thus it is that Boerhaave explains the digeſtion of ſoft food. With reſpect to that of a firmer texture, imagining, that the cauſes above recited are inſufficient to explain the digeſtion of them, he has recourſe to the triturating power of the ſtomach, produced by the action of the muſcular coat, and the pulſation of the aorta and the other adjacent arteries; the nervous fluid, which perhaps flows into the ſtomach more copiouſly than elſewhere; and laſtly, the continual and ſtrong compreſſion of the diaphragm and abdominal muſcles. In conſequence of theſe additional cauſes, in the firſt place, the food will be broken down into a pulp, and acquire a cineritious hue; ſecondly, the fibres, tendons, cartilages, &c. will be deprived of their juices while they retain their coheſion; thirdly, from vegetable and animal ſubſtances thus diſſolved, will be produced a fluid reſembling our humours.

CCXX. Thus has this celebrated phyſician explained his ideas concerning digeſtion in his Inſtitutions. He ſuppoſes, that there are two principal agents in this vital function, viz. the different fluids that are collected in the ſtomach, and the mechanical action of that organ; the ſecondary agents are heat, air, the nervous fluid, the remains of the food, and an incipient fermentation.

With reſpect to the gaſtric fluid, his ideas were indeterminate and unſettled. On comparing this paſſage with his Prælections it will appear, that he conceived that it acted in the ſolution of the food like a ſimple diluent, like water heated to the ſame degree. But facts without number related above ſhew, that it does not act in this manner, but as a a real ſolvent. That the ſolution is more ſpeedy and effectual than that obtained by mere water, appears from experiments equally numerous. Moreover, this fluid does not diſſolve ſoft and yielding ſubſtances only, but the hardeſt and moſt tenacious, contrary to Boerhaave's opinion.

With reſpect to trituration, the attentive reader will eaſily anticipate my anſwer. However remarkable the effects produced by the mechanical action of muſcular ſtomachs may be, intermediate and membranous ſtomachs have no ſuch power. I have made particular obſervations on the ſtomach of the dog, which ſo nearly reſembles that of Man, and it never appeared to have any motion ſufficient to break down the food. This was not only proved by thin tubes receiving no injury, but by inſpection of the ſtomach during the time of digeſtion (CXCIX, CC). The reader will find ſimilar proofs taken from the effects produced by my own ſtomach, in the CCIXth and CCXth paragraphs. Theſe direct arguments ſhew the inſufficiency of the Boerhaavian hypotheſis. It is beſides eaſy to ſhew its falſity, by examining the foundation on which it reſts. He deduces the triturating power from the action of the muſcular coat and the contiguous parts; but this coat is ſo thin in membranous ſtomachs, that its effects muſt needs be inconſiderable. Nor is the preſſure of the adjacent parts of much importance, at leaſt in the cat and dog;

for upon opening the abdomen and feeling the ſtomach, I perceived nothing but the pulſation of the arteries, as I had before done in ſome birds with muſcular ſtomachs (XXXVIII). But this pulſation does not compreſs the ſtomach. I likewiſe perceived by my touch, that this viſcus is affected by the vibrations of the neigbouring arteries; but the effects of theſe vibrations are not more conſiderable than the pulſation of its own arteries. The whole ſtomach was lifted up, and depreſſed by the motion attending reſpiration. The periſtaltic movement was alſo general in ſome caſes; but the former did not produce contraction, and the contraction produced by the latter was gentle, and incapable of triturating the aliment. It could only agitate, and thus diſpoſe it to be more readily diſſolved.

CCXXI. Heat, I readily agree with Boerhaave, in conſidering as a co-operating cauſe. My experiments prove its great importance. Though the gaſtric fluid is not inflammable (LXXXI, CXXIII, CXLIX, CLXXXV, CCXVI), yet it is diſpoſed by warmth to inſinuate itſelf into digeſtible ſubſtances, and reduce them to that gelatinous matter which ſerves immediately for nutriment. The ſame obſervation is applicable to menſtrua in general.

I willingly admit, that particles of air, while they are extricated from the food among which they are entangled by means of the ſaliva, contribute to its more ſpeedy ſolution.

But I cannot ſo readily allow, that digeſtion is promoted by the nervous fluid flowing copiouſly into the ſtomach; for its very exiſtence is uncertain, and the hypotheſis is altogether without foundation.

Much leſs leſs can I grant, that the remains of the aliment ſerve any ſuch purpoſe as he aſcribes to them. The great Haller juſtly obſerves, that our appetite and digeſtion are good only when the ſtomach is empty (*a*). I have had ſeveral opportunities of ſeeing this confirmed. When I fed a crow, a heron, or a falcon ſparingly, the ſtomach would be empty in ſix or ſeven hours; when they would take food again very greedily and digeſt it completely, as I found upon opening the ſtomach.

Whether an incipient fermentation contributes to digeſtion, according to the opinion of this writer, is a queſtion which ſhall be examined at ſome length in the following diſſertation, as it has been the ſubject of many modern experiments.

Laſtly, I muſt again differ from him with reſpect to fibres of fleſh, membrane, tendon, cartilage, bone, which, in his opinion, are not digeſted in the human ſtomach, but only have their juices expreſſed; for the experiments I made on myſelf prove, that the ſolid parts are really diſſolved, if we except only the hardeſt bones (CCV, CCVIII, CCXII, CCXIII, CCXIV). As Boerhaave endeavoured to reconcile the various opinions of phyſicians concerning digeſtion, he ſeems inclined to adopt in ſome meaſure the notion of thoſe who ſuppoſe, that the office of the ſtomach conſiſts in extracting the juice of animal and vegetable matters, among whom

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(*a*) Phyſ. T. 6.

Mr. Hecquet has particularly diſtinguiſhed himſelf. And a note to this paſſage, in which he obſerves, that the ſtems of hay are ſtill viſible in the dung of the horſe and the ox, notwithſtanding it is chewed ſo often by the latter, ſtill more clearly explains his idea. I conſidered it as of great importance to enquire, whether the ſame thing happens in animals belonging to other claſſes, which was really the caſe in ſome. We have ſeen, that the two ſpecies of crow above-mentioned are both granivorous and carnivorous. I ſometimes fed them with wheat a little bruiſed, and notwithſtanding they ſeemed to eat it greedily, their excrements conſiſted of dry fragments of this grain. This likewiſe happened when they had eaten tough fleſh. If I put the excrement in water and ſhook it briſkly, the greater part would be ſuſpended, but a little would fall to the bottom; this, upon examination, proved to be cellular ſubſtance with a few muſcular fibres, of which the particles cohered pretty firmly; the longeſt pieces meaſured about an inch. What remained ſuſpended in the water was more than twice as much as that which fell to the bottom, and ſtill retained the characters of fleſh. Young crows, which digeſt more ſpeedily than the adult (LXIX), do not completely digeſt tough meat. I could eaſily find cellular ſubſtance among their excrement; but when inſtead of hard they were fed with tender fleſh, and with ſome ſoft vegetable inſtead of wheat, the excrement did not ſhew the leaſt appearance of this ſort.

CCXXII. I made the ſame obſervation upon frogs. As theſe animals generally feed upon inſects, I often found among the excrement, when treated in the way juſt deſcribed, legs, thighs, and wings of locuſts, and the cruſtaceous parts of other inſects.

Leuwenhoeck, upon examining the excrement of the melvel, found, that it conſiſted of filaments reſembling the hairs of the beard cut off by the razor; theſe filaments he ſuppoſed to be the undigeſted remains of the fiſhes which the melvel had eaten (*a*). I can eaſily believe this to have been the caſe, eſpecially as it coincides with an obſervation of my own upon the excrement of the tench, in which, though I could not perceive any fleſhy fibres, yet the fragments of bone were diſtinctly viſible. I muſt however add, that though I have examined the fœculent matter of many other fiſhes with glaſſes of various magnifying powers, I could never diſtinguiſh the leaſt atom that had the characters of animal or vegetable matter. I have obſerved the ſame thing in that of nocturnal and diurnal birds of prey. The tough fleſh, of which a ſmall part was voided along with the excrement of crows (CCXXI), is entirely digeſted by the eagle, falcon, and owl. This obſervation may be extended to a multitude of birds of various kinds, of which, though I have preſerved the names in my journal, to avoid prolixity I will not now enumerate them. Serpents, though ſo ſlow of digeſtion, diſſolve their food ſo completely, that not a veſtige of any organized matter appears

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(*a*) Philoſ. Tranſact. n. 152. 1683.

in their excrements. This at leaſt I have ſeen in vipers, water and land-ſnakes.

Upon comparing my obſervations upon excrement with thoſe related by Boerhaave and others, I think it muſt be concluded, that conſidering animals in general, ſome ſubſtances of both kingdoms are voided unchanged along with their excrements, becauſe the gaſtric fluid is incapable of diſſolving them; but others are voided unchanged, only becauſe they do not continue long enough in the ſtomach to be digeſted. This is fully proved by my experiments on fleſh, membrane, tendon, and bone, the very ſubſtances of which Boerhaave ſuppoſed, that the ſolid parts were indigeſtible. Fleſh taken ſpontaneouſly by crows, part of which is voided undigeſted, but when kept many hours in the ſtomach is completely diſſolved, furniſhes another deciſive proof of the ſame propoſition.

It is ſurely not neceſſary to add, that I do not wiſh by theſe ſtrictures to leſſen the high reputation of the Dutch Hippocrates. Unprovided with experiments of his own, he collected the opinions of others, and framed a ſyſtem concerning digeſtion ſo ingenious and plauſible, that I willingly own that I formerly adopted it, and would not now reject it, if I was not compelled by concluſive experiments.

CCXXIII. I will conclude this diſſertation, with ſome remarks on a problem cloſely connected with reſearches concerning the efficient cauſe of digeſtion. Mr. Hunter, one of the beſt Engliſh anatomiſts of the preſent age, frequently in opening dead bodies, found the great curvature of the ſtomach conſiderably eroded, and ſometimes entirely diſſolved. In the former caſe, the edges of the wound were as ſoft as half-digeſted food, and the contents of the ſtomach had got into the cavity of the abdomen. The author obſerves, that ſuch a wound cannot have exiſted in life, having no connection with the diſeaſe, and more frequently appearing in perſons who have died violent deaths. In order to diſcover the cauſe of this phænomenon, he examined the ſtomachs of various animals, both immediately and ſome time after death. He obſerved in ſeveral the ſame appearance. Hence he thought he was enabled to aſſign the cauſe. He ſuppoſes the ſolution to be owing to a continuance of digeſtion after death, and that the gaſtric fluid is capable of diſſolving the ſtomach when it has now loſt its vital principle. From this diſcovery he infers, that digeſtion neither depends on the action of the ſtomach nor on heat, but on the gaſtric juices, which he conſiders as the true menſtruum of the food (*a*).

CCXXIV. When Mr. Hunter's ſhort but ſenſible paper came to my hands, I was engaged in experiments on digeſtion. I had diſcovered the primary importance of the gaſtric fluid in this proceſs, and that it acts out of the body; that is to ſay, in the dead body. I knew alſo, that after death this fluid iſſues from the coats of the ſtomach. From theſe data I had little difficulty in believing the fact related by the Engliſh anatomiſt, and adopting his explanation of it: nevertheleſs it was

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(*a*) Ph. Tranſ.

proper to repeat the experiment. Being unprovided with human ſubjects, I had recourſe to animals. Some were opened ſooner, and others later after death; but among the numbers I inſpected, not one had its great curvature diſſolved, or much eroded. I ſay, much eroded, becauſe I have often ſeen a little eroſion, eſpecially in different fiſhes, in which, when I had cleared the ſtomach of its contents, the internal coat was wanting. The injury was always confined to the inferior part of the ſtomach. If theſe facts are favourable to Mr. Hunter, a great number are againſt him. They do not however deſtroy his obſervations; mine are only negative, his are poſitive; and we know that a thouſand of the former do not deſtroy a ſingle one of the latter, provided it is well aſcertained. I have no reaſon to diſtruſt Mr. Hunter, for his paper has that air of ingenuouſneſs and candour which uſually accompanies truth.

CCXXV. The ill ſucceſs of my experiments did not induce me to abandon the idea of digeſtion after death, it only led me to conſider it in another point of view. If it be true, ſaid I to myſelf, that the gaſtric fluid exerts its action after death, it muſt produce ſome ſolution of the food. Let then an animal be fed and immediately killed, after ſome time let it be opened, and let us ſee whether the food has been at all digeſted. I determined to bring this obvious inference to the teſt of experiment; I therefore kept a raven faſting ſeven hours in order to empty its ſtomach, and then ſet before it an hundred and fourteen grains of beef, which were immediately eaten, and muſt have paſſed into the ſtomach, as this bird has no crop. I then killed it, and as it was winter, put it into a ſtove, where it was left ſix hours. Suppoſing this to be a ſufficient time for the gaſtric fluid to exert its action, I opened the ſtomach, and found the fleſh in the following ſtate. It was impregnated with gaſtric fluid, and was become tender; the colour was changed to a pale red, and the ſurface had a bitter taſte, while the internal parts retained the taſte of fleſh. After the gaſtric fluid was wiped away, it weighed only fifty-two grains; it had therefore loſt above half its weight in ſix hours, or, what amounts to the ſame thing, was above half digeſted. The pylorus, and the duodenum for about an inch, were occupied by an aſh-coloured mucus, which muſt muſt have been the diſſolved part of the fleſh.

At the ſame time I gave another raven, that had in like manner been kept faſting ſeven hours, an equal quantity of fleſh, and killed it in two hours and a quarter. My view was to obſerve the difference between what had lain ſix hours in the dead, and two and a quarter in the living ſtomach, and it was very great; for in this latter caſe the fleſh was totally diſſolved, except a few membranous pellicles, which I have found to be always longer in being digeſted than the muſcular fibres; the mucus was the ſame as before, only in larger quantity, and occupied more of the duodenum. Theſe two experiments compared together prove two things, firſt, that digeſtion continues after death; and ſecondly, that it is then far leſs conſiderable than in the living animal, though in the preſent inſtance the heat of the ſtove, which was about 100° (*a*), muſt have promoted it not a little. The heat of the living raven did not exceed 30° (*b*).

CCXXVI. Another dead raven was kept five hours in the ſame ſtove, after I had forced two dead lampreys, weighing together an hundred and twelve grains, down its throat. One lay in the œſophagus, the other had reached the ſtomach and was completely decompoſed, while the other was indeed entire, but ſoft and flaccid. This accident proves, that the gaſtric fluid is capable of producing a ſenſible degree of digeſtion at a time when the œſophageal juices are inert.

CCXXVII. Theſe experiments were made in winter. I determined to repeat them the next ſummer, becauſe then I could expoſe the dead animals to a greater heat. Accordingly in that ſeaſon ſome bruiſed veal was given to two ravens, which were immediately killed, and left ſeven hours in a window expoſed to the ſun. We have already ſeen in ſeveral paſſages, the influence of heat in promoting artificial digeſtion (CXLII, CLXXXVI, CCI, CCXVII). Nor did it now appear leſs conſiderable. Each raven had eaten ſixty-eight grains of fleſh, of which there was not an atom left entire; it was all diſſolved into the uſual gelatinous pulp, and the greater part had paſſed through the pylorus.

(*a*) Two hundred fifty-ſeven deg. Fahr. Ther.

(*b*) One hundred nine and an half ditto.

Theſe facts, I think, deciſively prove, that animals, at leaſt the ſpecies juſt mentioned, continue to digeſt after death. If we conſider the matter rigorouſly, it will be proper to obviate a difficulty that may be ſtarted. However careful we are to kill the animal immediately after it has ſwallowed food, it is certain, that there will be a ſhort interval between the time the food gets into the ſtomach and the death of the animal, and that the gaſtric fluids act upon it during this interval. Moreover, after death they will act for ſome time juſt as in life, ſince the vital heat is not inſtantly exſtinguiſhed. The digeſtion therefore obſerved in dead animals may, if not entirely, at leaſt in part, be produced by the gaſtric fluid acting during life, and a ſhort time after death.

Nothing could be more eaſy than to aſcertain the juſtneſs of this ſuſpicion, ſince we have only to thruſt a little food into the ſtomach of a dead and cold animal, and obſerve the conſequence. I made the experiment upon a raven that had been dead an hour, and had now only the temperature of the atmoſphere. Forty-two grains of beef cut into pieces were forced into the ſtomach, which was opened after the bird had lain ſeven hours expoſed to the ſun. And here inſtead of pieces of ſolid fleſh, I found only the uſual pulpy maſs, partly in the ſtomach and partly in the duodenum. The ſolution was therefore effected by the gaſtric fluid, independently of the powers of life.

CCXXVIII. The experiment was repeated upon an owl and a blackbird, which were killed immediately after meat had been given them, and left ſeven hours in a warm temperature. The fleſh given to the blackbird had been cut into three pieces, which together amounted to eighty-two grains; the owl had ſwallowed half an ounce and ſix grains in one piece. Upon opening the ſtomachs, I found the four pieces; but the ſurface was covered with a ſtratum of mucus, which ſhewed, that the fleſh had been diſſolved.

I thought, that perhaps if the fleſh had remained a longer time in the ſtomach it would be more digeſted; but this did not happen, at leaſt when I repeated the two preceding experiments under the ſame circumſtances, except that the birds were expoſed to the ſun for twenty-two hours, I could not perceive, that the ſolution of the fleſh was carried any further. The entrails emitted a putrid ſmell, but this was not the caſe either with the ſtomach or its contents.

CCXXIX. That I might be warranted in deducing general conſequences, I reſolved to repeat this ſingular experiment upon various claſſes of animals, and therefore had recourſe to fiſhes and quadrupeds. Of the former, the fiſh-market at Pavia only affords the pike, carp, barbel, tench, eel, and the like; but I took care to procure ſuch as were very freſh. I introduced into the ſtomach various animal ſubſtances, as little fiſhes, bits of veal and beef, frogs, grubs, &c. and opened them after an interval, ſometimes ſhorter ſometimes longer. I will give in a few words what is ſet down at great length in my Journals. The part of theſe ſubſtances that lay in the œſophagus, a poſition which they often had, was unaltered; this was ſometimes the caſe with that which had got into the ſtomach, but it was generally more or leſs eroded. A circumſtance reſpecting frogs deſerves to be mentioned. The tough ſkin of theſe animals was often deſtroyed, eſpecially at the bottom of the ſtomach; and where it ſtill remained it was ſo much ſoftened, that the ſlighteſt force was ſufficient to lacerate it. Hence it appears, that the gaſtric fluid of fiſhes retains its property of diſſolving fleſh, but in an inferior degree to that of birds, ſince it did not diſſolve ſo much.

CCXXX. The quadrupeds upon which I made theſe experiments, were dogs and cats. After keeping them faſting many hours, I gave them a certain quantity of fleſh, and then ſtrangled them without delay. Of three dogs and three cats, two of the former and as many of the latter were expoſed to the ſun for nine hours; the others were left in the ſhade. In the firſt the ſurface of the fleſh was gelatinous as uſual, but in the laſt this appearance was ſcarce perceptible. Theſe experiments confirm the utility, I ſhould rather ſay, the neceſſity of heat to digeſtion in many animals.

CCXXXI. To conclude this curious enquiry, I reſolved to ſee what change would take place upon fleſh when the ſtomach was taken out of the body. I made this experiment upon a cat, a raven, and an owl. Having fed them ſparingly, I cut out the ſtomach, and threw ligatures round the cardia and pylorus to prevent the contents from getting out. They were expoſed to the ſun in

a veſſel

a veſſel of water, leſt the heat ſhould dry them. In five hours and an half they were opened: the water had tranſuded through the coats; the ſurface of the fleſh was a little gelatinous, eſpecially in the ſtomach of the raven and owl; but the ſolution was trifling, in compariſon with that which took place when the ſtomach was left in the body. This was what might be expected, when the œſophagus no longer poured its liquor into the ſtomach.

In theſe experiments I did not perceive any eroſion of the ſtomach, any more than in thoſe made with the view of verifying Mr. Hunter's (CCXXIV). I only ſaw what I had ſeen before (ibid), a ſlight excoriation of the inferior part. We muſt therefore infer, that the coats of the ſtomach ſuffer leſs after death than fleſh introduced into it. I ſet before an hungry dog ſome pieces cut out of the ſtomach of another dog; he ate them without heſitation, and was killed immediately. After the body had lain in a warm ſituation nine hours, the ſtomach was opened. The pieces were ſenſibly diſſolved, but no change was produced upon the ſtomach of the animal, if we except the large curvature, which was ſo much macerated, that the villous coat might eaſily be rubbed off. It is, I think, not difficult to aſſign the reaſon, why the ſtomachs of dead animals are not liable, like their contents, to be diſſolved. Theſe bodies are inveſted on all ſides by the gaſtric fluid, whereas it acts only on the internal ſurface of the ſtomach.

Upon reviewing the experiments related in the CCXXVth and following paragraphs it cannot, I think, be doubted, that digeſtion goes on for ſome time after death. I therefore entirely agree ſo far with the celebrated Engliſh anatomiſt, but I cannot with him ſuppoſe, that this function is independent of heat (CCXXIII); numberleſs facts related in this work fully prove the contray.

DISSERTATION VI.

WHETHER THE FOOD FERMENTS IN THE STOMACH.

CCXXXII. I WILL now, agreeably to my promiſe in the foregoing diſſertation (CCXXI), enquire whether the food ferments in the ſtomach. This opinion was almoſt univerſally adopted by phyſicians about the middle of the laſt century, an æra at which the explanation of the various functions of the human body was ſought in fermentations of various kinds, as it had before been in a ſubtile matter, as it has ſince been in electricity, and is at preſent in divers ſorts of elaſtic fluids. This notion was afterwards combated among others by Boerhaave, who found, by direct obſervations, that this multiplicity of fermentations did not exiſt in nature, but was merely the ſuggeſtion of fancy. Of the numberleſs modifications of this proceſs, which phyſiologiſts had imagined, he admitted only that very limited and imperfect one, which, according to him, takes place in the ſtomach. The food in the ſtomach of animals, and particularly of Man, is, in his opinion, in circumſtances highly favourable to fermentation. The ſaliva and the gaſtric fluid ſerve inſtead of water; the free acceſs of air, the cloſeneſs and heat of the ſtomach, the nature of the food itſelf neceſſarily produce fermentation, as is farther evident from the eructations conſequent upon taking food, and the rumbling noiſe frequently heard in the belly. But the ſhort continuance of the food in that viſcus, and other cauſes, prevent the proceſs from being ever carried to its utmoſt pitch.

CCXXXIII. Thus far only, according to Boerhaave and his followers, does the fermentation of the food proceed in the ſtomach. This limitation has been thought too great by Dr. Pringle and Dr. Macbride, two celebrated modern phyſicians. They find no difficulty in ſuppoſing, that a complete fermentation takes place in digeſtion, and that it is the chief agent in this important function. In their reſearches on this ſubject, they have endeavoured to imitate the operations of nature out of the body. They took various animal and vegetable ſubſtances, ſuch as are uſed every day for food; they placed them both by themſelves, and mixed with ſeveral other ſubſtances in a warm temperature, adding a quantity of water or ſaliva. Under theſe circumſtances they found, that they ſooner or later began to ferment; that this proceſs afterwards ran very high, then abated, and at laſt ended in the decompoſition of the ſeveral ſubſtances, which acquired alſo a ſweet taſte. Theſe different gradations of fermentation were evident from the ſwelling, rarefaction, and inteſtine movement of the maſs, from the generation of a multitude of air-bubbles, and from the ſubſtances which at firſt ſunk to the bottom, at length floating on the ſurface of the fluid. Theſe experiments firſt made by Pringle, and afterwards repeated and varied by Macbride, determined them both to conſider digeſtion as a proceſs merely fermentative. Their theory is as follows. The food divided by maſtication and penetrated by the ſaliva, begins as ſoon as it gets into the ſtomach to be agitated by that inteſtine movement which always accompanies fermentation; this movement is excited by the warmth of the place, by old remains of food, by the gaſtric fluid, and above all by the ſaliva, which is particularly adapted to produce and promote this proceſs. The firſt effect of the inteſtine commotion will be to raiſe the ſolid parts of the aliment to the ſurface of the gaſtric liquor; here they will be ſuſtained for ſome time by the air-bubbles; but on their ceſſation they will fall down again and be thoroughly incorporated with the fluids of the ſtomach. The periſtaltic motion, the alternate preſſure of the diaphragm and abdominal muſcles, and the continual pulſation of the adjacent large veſſels will render this mixture ſtill more complete. In ſuch a ſtate the food paſſes into the ſmall inteſtines, where the fermentative motion produces ſtill greater changes in conſequence of the mixture of the bile and pancreatic juice. And now the various kinds of food are changed into a ſweet, mild, nutritious matter, which ferments briſkly, and is denominated *chyle*. In conformity with this theory, theſe phyſicians eſtabliſh a new ſyſtem of great importance, according to them, in the practice of medicine. It is ingeniouſly explained by Pringle, in his Appendix containing Experiments on ſeptic and antiſeptic Subſtances, and by Macbride in his experimental Eſſays on the Fermentation of alimentary Mixtures.

CCXXXIV. The opinion of theſe two modern writers have been adopted by many phyſiologiſts, while others have ſtill adhered to the doctrine of Boerhaave, concerning an incipient and incomplete fermentation only taking place in the ſtomach; ſo that on this ſubject the phyſicians of Europe ſeem to be divided into two ſects. When I read Pringle and Macbride, I had only made a few experiments on the digeſtion of ſome animal and vegetable ſubſtances encloſed in tubes by gallinaceous birds (XXXIX, XL, XLI, XLII, XLIII); and I began to perceive, that the gaſtric juice acted as a menſtruum upon the food. But I could not learn from theſe experiments, whether fermentation takes place at the time they are diſſolved. As indeed the gaſtric fluid is a ſolvent, it may act independently of fermentation; chemiſtry affords numerous inſtances, in which there is no token of fermentation during the decompoſition of the ſolvend. But there is no abſurdity in ſuppoſing, that an inteſtine fermentative motion is generated in the mixture, at the time the gaſtric fluid diſſolves the aliment. And in this caſe, fermentation would accompany digeſtion, though it would not according to the doctrine of Pringle and Macbride (CCXXXIII), be the efficient cauſe.

In order to obtain information concerning this phænomenon which I had not noticed, I had recourſe to further experiments. As the theory in queſtion is entirely founded on the fermentation of animal and vegetable matters in veſſels, I ſet in glaſs phials bread, fleſh, and ſaliva; bread, fleſh, and water; flour, ſaliva, and fleſh; for in theſe mixtures the writers above-mentioned obſerved the moſt rapid fermentation. The phials were ſtopped, and ſet in a place where the heat amounted to 20°—24° (*a*). The mixtures began, ſome ſooner and others later, to emit air-bubbles, which ſoon increaſed in frequency and ſize; the ſurface of the liquor was covered with froth, which continued as long as the air was ſeparated. During this time the maſs ſwelled greatly, the inteſtine commotion was manifeſt, and the ſubſtances immerſed being made ſpecifically lighter by the air-bubbles that adhered to them and the increaſe of bulk, roſe to the ſurface of the fluid. Here then the tokens of fermentation were apparent, and ſo far I entirely agree with Pringle and Macbride.

CCXXXV. But ſound logic forbade me to allow ſo readily, that the ſame proceſs takes place in the ſtomach. I had indeed many reaſons for withholding my aſſent. Not to mention the ſhort continuance of the food in that viſcus, a circumſtance which did not eſcape Boerhaave (CCXXXII), I conſidered, that although the ſaliva produces and promotes fermentation, the gaſtric fluid may not

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(*a*) Seventy-ſeven and eighty-ſix deg. of Fah. Ther.

have this property. Though the gaſtric fluid conſiſts in part of ſaliva, yet as there are ſeveral other ingredients, a compound muſt be formed with properties different from thoſe of its conſtituent parts. I have adduced many inſtances to prove, that the gaſtric fluid retains in ſome meaſure its ſolvent power out of the body; but the ſaliva never exhibited any ſuch property. I have already ſhewn, and ſhall ſtill more clearly ſhew in the ſequel, that fleſh immerſed in the gaſtric fluid is not liable to putrefaction; but when put into ſaliva, it putrefies ſooner than in water. This was one of my motives for not immediately adopting the ideas of Pringle and Macbride. It were to be wiſhed, that theſe phyſicians had made trial of the gaſtric liquor alſo, before they concluded, that what they obſerved in veſſels takes place likewiſe in the ſtomach; nor can I well conceive, how they both came to overlook a circumſtance of ſo great importance. Moreover we know, that reſt is neceſſary to fermentation; but the ſtomach, beſides the motion of the whole body, has a movement peculiar to itſelf. Laſtly, ſhould fermentation once begin, it muſt in all likelihood be ſoon ſtopped by the freſh ſaliva and gaſtric liquor that are running perpetually, and in no ſmall quantity, into the ſtomach. Theſe two laſt objections have been already ſtarted, though nobody, as far as I know, has taken the pains of verifying them by experiment. But as the queſtion could be decided in this way only, I determined to undertake to ſupply the omiſſion.

CCXXXVI. I have already ſpoken of artificial digeſtion in ſeveral paſſages. Experiments of this kind afforded me an excellent opportunity of obſerving, whether the ſolution of fleſh out of the body was accompanied by fermentation, and I never failed in a ſingle inſtance to attend to this circumſtance. I found, that when the veſſels remained at reſt, a few ſmall air-bubbles began to ariſe in the ſpace of a few hours; they afterwards became larger and more frequent, and adhering to immerſed ſubſtances, cauſed them to riſe to the ſurface of the liquor. This air was either entangled in the mixture, or, according to Pringle and Macbride, formed part of it, was extricated, and rendered elaſtic by the heat, or what ſeems more probable, came from both theſe ſources. The mixtures either ſunk again or continued to float, while they were diſſolved by the gaſtric menſtruum; not the ſlighteſt inteſtine motion was ever perceptible, juſt contrary to what happens when ſaliva is employed. If I now and then ſhook the veſſel a few hours after making the infuſion, very few air-bubbles were generated, and the mixture hardly ever roſe to the ſurface, though it was juſt as well diſſolved as when the veſſel remained at perfect reſt. I find in my Journals, that I agitated the veſſels upon fourteen ſeveral occaſions without obſerving the ſmalleſt difference in the reſult of the experiment. I could not therefore allow, that fermentation was the efficient cauſe of theſe artificial digeſtions, nor even that it was a concomitant circumſtance, or an effect; and freſh experiments inclined me more and more to reject this opinion. I have already mentioned the great abundance of gaſtric fluid in crows, and the facility with which they digeſt their food, more eſpecially neſtlings (LXIX, LXXXIII). Among the various trials I made with this fluid out of the body, I endeavoured to renew it, as it is renewed in the ſtomach. Several glaſs tubes were filled with it to a certain height, and ſuſpended in a vertical poſition; into the upper extremity a ſmall funnel was put; ſome gaſtric fluid was poured into it from time to time, the narrowneſs of the orifice of the funnel allowed it to fall only drop by drop into the tubes. The lower extremity of the tubes was not cloſely ſtopped, that nearly as much might run out below as fell in from above. Matters being thus arranged, I immerſed in the tube fleſh and bread, both by themſelves and mixed together. The ſolution was exceedingly ſpeedy, on account both of the warmth of the atmoſphere, and the conſtant renewal of the gaſtric fluid. Notwithſtanding the tubes remained at perfect reſt, only a few air-bubbles were diſcharged; not the leaſt inteſtine motion could be perceived; the fleſh and bread fell immediately to the bottom, and remained there till they were gradually incorporated with the gaſtric fluid: in ſhort, they were digeſted without a ſingle circumſtance occurring that uſually attends fermentation.

CCXXXVII. If this proceſs does not take place out of the body, it ſeems highly improbable that it ſhould within; however, to be certain of this, it was proper to conſult the ſenſes. Is digeſtion, according to Pringle and Macbride, a fermentative proceſs? Let us then inſpect it while it is going on, by ſurprizing Nature in her operation, and ſeeing in what it conſiſts. With this view, I gave four hens that had been kept faſting twelve hours, ſome wheat, and in five hours opened the gizzard without killing them. This method I practiſed in the following experiments, being apprehenſive leſt opening the animal after death might not anſwer the end I had in view. Both gizzards were full of grains of wheat moſtly broken, and mixed with a ſemifluid farinaceous paſte. The orifice of the pylorus and great part of the duodenum was full of the ſame paſte, which had not in this caſe much fluidity. Upon examining this paſte, both with my naked eye and the microſcope, I could not perceive any ſign of fermentation; the parts were at perfect reſt, and entirely free from air-bubbles. I waited three hours longer before I opened the gizzards of the other two ducks, in order to ſee whether what had not taken place at the beginning of the proceſs, might not have happened when it was further advanced. In this caſe, the paſte was more diluted with gaſtric liquor, and of the grains of wheat little was left but the bran; I obſerved no more inteſtine motion or air-bubbles than before.

CCXXXVIII. My next experiment was made upon three ravens that had not yet quitted the neſt. Two hours after I had fed them with beef, I opened the ſtomach of one of them. The fleſh was half diſſolved, but I could not perceive any ſign of fermentation. I made the ſame obſervation upon the two others, which were opened an hour and three quarters afterwards, notwithſtanding

ſtanding the digeſtion was complete; for nothing remained in the ſtomach but a denſe grey fluid, conſiſting of fleſh diſſolved in the gaſtric fluid.

Of animals with membranous ſtomachs I examined an owl, ſeveral dogs, cats, and land and water-ſnakes, endeavouring always to make my obſervations at three diſtinct times, at the beginning, towards the middle, and at the end of digeſtion. But at no time did I perceive any tendency to fermentation. In one dog and one cat only did I obſerve a few air-bubbles among the food after it was completely digeſted; but there was not the leaſt inteſtine motion perceptible. Serpents, animals ſo ſlow of digeſtion, were well adapted to ſhew the progreſs of this function; but neither did they form an exception to the general obſervation. Theſe facts obliged me to reject the opinion of the Britiſh phyſicians and their followers; nor do I know whether that of Boerhaave is admiſſible, who, while he excludes a complete, infers an incipient fermentation, from the eructations that ariſe after taking food (CCXXXII); but this may be occaſioned by the rarefaction of the air entangled among the aliment, by the mere heat of the ſtomach.

CCXXXIX. Modern chemiſts have diſtinguiſhed three ſpecies or degrees of fermentation, the vinous or the ſweet, the acetous, and the putrid. As they eſſentially conſiſt in an inteſtine motion excited by heat and a proper degree of moiſture (*a*), and as

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(*a*) Macquer Dict. Art. *Fermentation*.

no ſuch motion can be ſeen in the food in the ſtomach, it follows, that not even the vinous, much leſs the acetous or putrid takes place in digeſtion. It remains to be enquired, whether this function is connected with a principle of acidity, as ſome ſuppoſe, or of putrefaction, according to others. I ſhall ſtate the facts which appear to favour each of theſe opinions. In behalf of the firſt, its advocates adduce acid eructations and vomitings from the human ſtomach, that diſagreeable acid ſmell which is exhaled from the ſtomachs of granivorous birds and ruminating animals, the acetous taſte of the internal coat; the diminution of the bulk of the contents of the ſtomach of man and animals, not to mention other arguments that may be ſeen in modern phyſiologiſts, and eſpecially in Haller.

CCXL. The prodigious number of ſtomachs I have opened, have afforded me opportunities of acquiring full information on this point. In animals ſtrictly carnivorous, ſuch at birds of prey and ſerpents, the food never appears acetous, either to the taſte or ſmell during the time of digeſtion. The ſame obſervation will apply to frogs and fiſhes; and it may be extended to omnivorous animals, ſuch as crows, when they feed upon fleſh; but the pultaceous maſs reſulting from vegetables, and in particular from bread, now and then acquires a ſlight acidity. I have found the ſame taſte in two dogs, and more frequently in herbivorous animals, ſuch as ſheep and oxen; and alſo in thoſe which are at once herbivorous and granivorous, viz. in the gallinaceous kind; and in the laſt-mentioned claſs, not only the food in the ſtomach had an acetous taſte, but that in the craw likewiſe. In the third diſſertation will be found ſome inſtances of this (CXXXIX, CXL, CXLI, CXLIII). With reſpect to Man, I will relate what has happened to myſelf. During the whole month of May and great part of June, I eat ſtrawberries with ſugar and white wine at dinner and ſupper. From this agreeable mixture I never experience any inconvenience in the day-time; but by what I eat in the evening my ſleep is frequently diſturbed; the contents of my ſtomach riſe almoſt into my mouth, and then fall back again, leaving a moſt diſguſting ſour taſte behind. This unpleaſant circumſtance does not, however, prevent me from recovering my reſt, and digeſting my food perfectly. I have beſides many times been ſubject to a like diſagreeable ſenſation, after eating too much fruit in ſummer and autumn. Every man muſt ſome time or other have been ſenſible of his meat and drink having turned ſour.

CCXLI. I was further deſirous of knowing, whether the acid principle ſometimes found in the ſtomach, is capable of diſſolving calcareous earth, and ſuch other bodies as acids act upon. I accordingly gave ſome carnivorous birds pieces of coral and ſea-ſhells, and they were thrown up without any change of colour, or diminution of weight. This was what might be expected. I next made the ſame experiment on a hen and a turkey, which were killed in two days. The coral and ſhells were very much corroded, and the former was reduced to pieces; but a moment's ment's reflection ſhewed me, that the corroſion might be owing to the action of the ſtomach, and not to an acid. The doubt, however, might eaſily be removed, by encloſing the ſame ſubſtances in ſtrong metallic tubes. The reſult of ſeveral experiments made in this manner was, firſt, that the pieces of coral and ſhell were almoſt always diminiſhed; but the diminution ſcarce ever exceeded three or four grains: ſecondly, that the ſurface was ſoftened; and thirdly, that it was turned black, eſpecially in the coral. I immerſed at the ſame time the ſame ſubſtances in diluted vinegar, and as ſimilar effects were produced, and particularly the black colour, I inferred, that the phænomena roſe from a like cauſe; laſtly, I repeated the experiment on myſelf. The tubes were covered as before (CCVIII), to prevent any feculent matter from getting into them. They were all voided without inconvenience. When I ate fleſh, with the addition only of a little bread, the ſubſtances contained were neither diminiſhed nor altered in their colour. But upon eating a large quantity of different vegetables; the coral and ſhells were generally diminiſhed and darkened. Theſe facts prove the preſence of an acid principle in the ſtomachs of ſome animals, and man himſelf. It is, however, not perpetual, but depends on the quality of the food.

CCXLII. This acid ſoon diſappears. I gave ſeveral gallinaceous fowls ſome bread at the ſame time. The ſtomachs were examined at different intervals, viz. two, three, three and an half, four, and five hours afterwards. As long as the bread preſerved its conſiſtence, it was frequently acid; but as ſoon as it was reduced to chyme this taſte was totally loſt. Nor could I perceive the leaſt ſign of it in that which had paſſed into the duodenum. I made upon myſelf the following obſervation.

When the unpleaſant acid taſte mentioned above (CCXL) came into my mouth in conſequence of eating ſtrawberries, I kept myſelf awake twice during the remainder of the night. Acid eructations continued to ariſe for ſome time; they at laſt ceaſed; yet from a ſenſe of weight I knew, that the contents of the ſtomach were not entirely digeſted; but the flatulence that came from them had no longer the ſlighteſt acidity.

CCXLIII. But what produces this acidity in the ſtomach? Does it come from the gaſtric fluid, or from the food? There are good grounds for rejecting the former, and admitting the latter of theſe origins. In the firſt place, this acidity does not appear on all occaſions; I never could obſerve it ariſing from fleſh. Now if it came from the gaſtric fluid, why ſhould it not be communicated to every kind of food, ſince every kind is alike impregnated with it? Secondly, when I ate vegetables, the effects of an acid in my ſtomach were apparent, but not when I ate fleſh (CCXLI). Thirdly, when vegetable food is completely diſſolved by the gaſtric fluid, it then loſes all acidity (CCXLII). Laſtly, if acid bread be encloſed in tubes and given to a crow, when it is thrown up four or five hours afterwards, the little that remains inſtead of being ſour, is now turned ſweet.

CCXLIV. Notwithſtanding theſe proofs, that this acidity is not owing to the gaſtric fluid, but to the tendency the food itſelf has to turn ſour whenever it is in a warm temperature, is it not ſuppoſed, that this fluid both in Man and animals is of an acid nature? Have not moſt of the antient and many modern phyſicians ſubſcribed to this opinion? I ſhould therefore have incurred the reproach of negligence, if I had not undertaken a chemical analyſis of it. The gaſtric fluid of every animal mentioned in theſe diſſertations, not excepting my own, was ſubmitted to the following experiments. Having taken the precautions above deſcribed (LXXXI, CCXV), to procure it in a ſtate of purity, I dropped it upon ſalt of tartar *per deliquium*, and into the nitrous and marine acids, without ever perceiving any change of colour, any motion or efferveſcence; whence I was obliged to infer, that the gaſtric fluid is neither acid nor alkaline, but neutral. I thought it would alſo be proper to ſubject thoſe kinds which could be procured in large quantity, as that of the crow, to the action of fire; I therefore entreated my illuſtrious colleague and friend, Counſellor Scopoli, to undertake the analyſis, as he was not only provided with the proper apparatus, but eminent for his ſkill in chemiſtry, of which ſcience he is deſervedly public profeſſor. He complied with my requeſt, and in a few days favoured me with the following account.

Chemical Analyſis of the Gaſtric Fluid of the Crow.

"The liquor is turbid, and of a darkiſh colour. When ſhaken it emits a ſmell rather diſagreeable.

When triturated with quick-lime or ſalt of tartar, a fetid urinous odour is exhaled.

It does not effervеſce with either of the mineral acids. It gives rather a green hue to ſyrup of violets.

Two drachms expoſed to a gentle heat left a dark-coloured ſediment weighing two grains, which attracted the humidity of the air. This reſiduum had a nauſeous ſmell. It did not effervеſce with acids.

I next filtered and diſtilled it. A darkiſh matter was left upon the filter, which, when it was dried, appeared in the form of a nut-brown powder, of a ſalt and bitter taſte.

The liquor which paſſed into the receiver was divided into five portions. The firſt had a ſlight taſte, and an empyreumatic ſmell. The ſecond had a ſtronger taſte and ſmell. The third, fourth, and fifth reſembled the ſecond, but the laſt had the ſtrongeſt empyreuma.

The belly of the retort was almoſt entirely covered with a white ſaline ſubſtance, which upon being triturated with quick-lime emitted a fetid urinous ſmell. In the bottom there remained a tough dark-coloured ſubſtance, reſembling an extract. It did not effervеſce with acids; its ſmell was empyreumatic,

matic, and its taſte ſalt, bitter, and nauſeous. This ſalt is neither acid nor alkaline, for it does not efferveſce either with acids or alkalies; but when a little oil of tartar *per deliquium* is mixed with it, it emits a penetrating urinous odour, exactly like that of ſal ammoniac.

From theſe experiments we may conclude, that the gaſtric fluid contains, firſt, a pure water; ſecondly, a ſaponaceous and gelatinous animal ſubſtance; thirdly, ſal ammoniac; fourthly, an earthy matter like that which exiſts in all animal fluids.

The ſaponaceous ſubſtance altered by fire emits that unpleaſant empyreumatic ſmell.

The ſal ammoniac being enveloped by the ſoapy matter does not ſublime, as it does when not entangled by other ſubſtances.

The gaſtric fluid of the crow precipitates ſilver from nitrous acid, and forms luna cornea. This phænomenon might induce us to ſuppoſe, that common ſalt exiſts in the gaſtric fluid; but as the ſalt contained in this fluid is not common ſalt but ſal ammoniac, we muſt ſuppoſe, that the ſilver is ſeparated from the nitrous, on account of its ſtronger attraction for the marine acid, which alſo far exceeds the attraction of the volatile alkali for the latter acid.

I wiſh you would repeat theſe obſervations on the gaſtric fluid of animals feeding only on vegetables. If in this alſo ſal ammoniac ſhould be found, we muſt conclude, that the marine acid is generated by the animal powers; and we might ſuſpect, that the marine acid of ſea ſalt is produced by the animals that inhabit the ocean. This is however a mere conjecture.

I am, &c.

SCOPOLI."

A little after I had received this account from my celebrated colleague, I quitted Pavia, to ſpend the ſummer vacation in my own country, where I had no opportunity of making experiments on the gaſtric fluid of any animal ſtrictly herbivorous, though I earneſtly wiſhed for it. I obtained, however, ſatisfactory proofs from the raven, that the ammoniacal ſalt does not depend on animal food, but on the powers of life. I fed five ravens for fifteen days on vegetables alone, and then by means of ſpunges procured a quantity of gaſtric fluid, which I ſuppoſed would have no properties that could be aſcribed to animal food. When I made with it the experiments deſcribed above, it did not appear to be acid or alkaline; it had a ſalt taſte, and upon pouring a few drops into a ſolution of ſilver in the nitrous acid, luna cornea was precipitated. There is therefore every reaſon to ſuppoſe, that if this fluid was diſtilled, ſal ammoniac would be obtained; and therefore, that the marine acid is the product of the animal powers. But whatever we are to think either of this or the other ingenious conjecture of my colleague, which have indeed little connection with our preſent enquiry, it is certain, both from his and my experiments, that the gaſtric fluid is not either acid or alkaline, but neutral.

CCXLV. But I muſt not conceal thoſe arguments which are adduced to prove, that there is a latent acid in this fluid, though it cannot be detected by any of the ordinary chemical means. It is well known, that a ſmall quantity of acid will curdle milk, an effect produced in the ſtomach of animals, and of ſucking calves in particular, in which caſe we cannot ſuſpect any vegetable acid to be preſent; the phænomenon muſt, therefore, be attributed to the latent acidity of the gaſtric fluid. And as it is continually ſecreted by the internal coat of the ſtomach, we cannot be ſurprized, that this coat in ſome animals ſhould retain the property of curdling milk. This is well known to cooks, who, when they have no rennet, take the innermoſt coat of the ſtomach of a fowl and ſteep it in water; which water, when thus impregnated with the juice of the ſtomach, will ſerve for turning milk as well as rennet itſelf.

Hence ſome have ſuppoſed, that the ſtomach contains a latent acid. My firſt ſtep was to aſcertain the fact. I therefore triturated the internal coat of a hen with water, which was thus rendered turbid, and in an hour and a half curdled a quantity of milk. The ſame effect was produced by the internal coat of other gallinaceous birds, viz. the capon, turkey, duck, gooſe, pigeon, partridge, quail, treated in the ſame manner. I further diſcovered, that this property belongs alſo to intermediate and membranous ſtomachs, by experiments on that of the crow, heron, birds of prey, the rabbit, the dog, cat, various reptiles, and ſeveral ſcaly fiſhes. In theſe trials the ſtomachs were freſh. I next tried dried ones, chiefly taken from the gallinaceous claſs, which being almoſt of the conſiſtence of horn, become dry in a very ſhort ſpace, and at the ſame time exceedingly brittle. The reſults were the ſame as before. Nor did it make any difference, if they had been kept ever ſo long. I have had for three years the internal coats of the ſtomach of ſeveral fowls, and upon triturating them with water, while I am writing, they curdle milk as well as at firſt. If they are pounded and mixed with milk, they anſwer the purpoſe equally well.

CCXLVI. But is this property confined to the internal coat? It was eaſy to determine this by treating the others in the ſame manner. The nervous coat has this property in ſome degree, but falls far ſhort of the internal. Whether cut into ſmall pieces and macerated in water, or mixed immediately with milk, the effect is not ſo ſpeedily produced, nor ſo conſiderable, nor are the curds ſo hard. The muſcular and cellular coats have not this property in the ſmalleſt degree, at leaſt in gallinaceous birds, upon which theſe experiments were made. Hence it would ſeem, that it reſides in the internal coat ſolely; for the effects produced by the nervous coat, may be owing to its lying in contact with the former.

CCXLVII. But is this property inherent in the internal coat, or adventitious, and owing to the gaſtric fluid with which it is impregnated? I incline to the latter opinion, ſince the gaſtric fluid ſo readily curdles milk. I ſhould weary my reader, was I to recount all my experiments. I will therefore only ſay, that the gaſtric fluid, from whatever animal it was obtained, poſſeſſes this property, whether I procured it by ſpunges, by opening the ſtomach and expreſſing it out of the glands, and the mouths of the little arteries, with which this viſcus in general abounds. I have further found, that the gaſtric fluid need not be freſh. That of crows, at leaſt, preſerves its virtue for three months.

CCXLVIII. But is it a neceſſary conſequence of theſe experiments, that the gaſtric fluid contains an acid? As no chemical teſt ſhews this quality, there can be no juſt motive to admit it, unleſs it can be proved to be a neceſſary conſequence of the curdling of milk. This is maintained by the illuſtrious Macquer among others, who is of opinion, that whatever bodies of the animal and vegetable kingdom coagulate milk, have either a manifeſt or occult acidity (*a*).

The foundation of this opinion is the common obſervation, that acids are the ſole cauſe of the curdling of milk. To this reaſoning, I ſhall only oppoſe a ſingle fact: I have diſcovered, that though ſeveral animal ſubſtances are incapable of producing this effect, yet others have this property. Thus for inſtance, if the blood or bile of a turkey be mixed with milk, it will retain its fluidity; but

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(*a*) Art. Milk.

pieces of the heart, liver, or lungs of that bird, will curdle it readily. This obſervation is not merely owing to accident; I have made the experiment repeatedly with different turkies, and always with the ſame ſucceſs. If therefore the coagulation of milk be always owing to an acid, we muſt ſuppoſe an acid in the heart, liver, and lungs of the turkey. I am not ignorant, that many chemiſts, in oppoſition to the Boerhavian ſchool, think a real acid exiſts in the different parts of animals, and particularly in the blood; but according to this hypotheſis, I cannot comprehend, why the blood of the turkey and other animals does not coagulate milk. With reſpect to the latent acid of the gaſtric fluid, I ſhall very willingly leave my readers to adopt what opinion they ſhall think moſt probable. The milk I employed for my experiments, was ſometimes that of the ſheep, but generally of the cow. It curdles ſpontaneouſly, as every one knows, ſooner or later, according to the temperature of the atmoſphere. When I mixed it with gaſtric juice, or any other fluid, I always left another portion by itſelf. In the former caſe, the coagulation ſoon took place without any ſign of acidity, whereas milk alone required ſeveral hours, and ſometimes a day or two, and the coagulum had always an acid taſte.

CCXLIX. But it is time to conſider the reaſons adduced by others, to prove that digeſtion is attended with an incipient putrefaction. Theſe reaſons are founded upon facts related by different authors, and detailed in their order by Haller, in his great work (*a*). Nothing, according to them, can be more evident, than the ſigns of putrefaction during digeſtion. The ſtomach of a hyena and of a ſerpent, have been obſerved to emit an intolerable ſtench. The breath of the lion and eagle is very fœtid, as alſo that of the dog, when digeſtion has been prevented by the exhibition of opium. A dog without taking opium, was obſerved to admit an odour of excrement from his ſtomach; the food in the ſtomach of birds has nearly this ſmell. The ſame obſervation has been applied to fiſhes; and the inſtance of a dog-fiſh has been adduced, of which the ſtomach was full of a fœtid jelly, that contained the food diſſolved. The contents of the human ſtomach ſometimes become fœtid. Vegetable ſubſtances alſo degenerate into a putrid maſs, when they continue long in the ſtomach, as appears from the putrid ſmell they exhale, the green colour they impart to tincture of mallows, and the alcaline principle they afford on diſtillation.

After having related theſe facts, the Swiſs phyſiologiſt proceeds to give his own opinion. He thinks, that in digeſtion there is only an incipient, not a complete putrefaction; which only takes place when the food remains a long time in the ſtomach, as is evident from the facts juſt mentioned. He alſo ſuppoſes, that the change produced by the digeſtive powers, eſpecially in the human ſtomach, approaches nearer to putrefaction than aceſcen-

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(*a*) T. 6.

cy; this he infers, from the putrid ſmell that exhales from fleſh found in the ſtomach of ſome animals, notwithſtanding there has been no impediment to digeſtion (*a*). This opinion, adopted before Haller by Boerhaave (*b*), has moreover been received by two celebrated writers, Gardana (*c*), and Macquer (*d*).

CCL. Notwithſtanding the reſpectable authority of theſe writers, I do not think the facts adduced ſufficient to perſuade an impartial philoſopher; they are not only too few, but were obſerved by mere accident; nor had the obſervers the ſmalleſt intention of entering into a full diſcuſſion of this point.

Though the time requiſite for digeſtion in different animals is different, yet in many it does not exceed five or ſix hours, and in ſome is ſtill ſhorter. Now it ſeemed proper to examine what change fleſh ſet to putrify, would undergo in that ſpace of time; I therefore took ſome freſh veal cut into ſmall pieces, and put it in a phial of water, which was ſtopped with paper. The phial was put into a ſtove, where the mercury roſe to between 30 and 35°.

About the beginning of the fourth hour, the fleſh had loſt its red colour, and was turning blue. It was alſo become flabby, but for nine hours it had no putrid ſmell. Mutton and beef, in ſeveral trials, did not anſwer to

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(*a*) L. c.

(*b*) Chem. T. 2.

(*c*) Eſſai pour ſervir à l'Hiſtoire de la Putrefaction.

(*d*) Art. common Salt.

this time exactly, but no bad ſmell was ever perceptible for eight hours. Theſe experiments ſhew, that fleſh eaten by many animals, among which Man may be enumerated, has not time to run into the putrefactive fermentation, eſpecially as the temperature of animals is lower than that to which theſe ſeveral ſorts of meat were expoſed. However, for greater certainty, I made the following trials. I have before mentioned introducing into the ſtomachs of crows, pyriform glaſs veſſels, of which the ſmall end was open, and came out at the mouth (LXXXIX).

I now took two of them, and putting ſome beef, with a little water, into one, and ſome veal into the other, forced them down the throat of the crows. In order to examine the ſtate of the fleſh, I now and then drew them up, and immediately returned them. Between the ninth and tenth hours, the beef emitted an odour, which though it could not be called putrid, was diſagreeable. At the expiration of the tenth hour, there was a diſtinct putrid ſmell that became gradually ſtronger and ſtronger. In a day the fleſh turned livid, acquired a nauſeous taſte, and the particles began to ſeparate. The ſame appearances took place rather ſooner in the veal. It therefore appears, that fleſh in the heat of this ſpecies of bird requires a longer time to putrify, than to be digeſted. After the glaſs veſſels were taken out of the ſtomach, I gave one of them the ſame quantity of beef and veal; and upon opening the ſtomach in three hours, found that it was entirely conſumed.

CCLI. Theſe experiments prove, that no putrid tendency is ever acquired by meat during digeſtion. Nor did I ever perceive any ſuch tendency in food lying in the ſtomach (LXV, CCIX); yet as I had never made experiments for this expreſs purpoſe, and as ſome phyſiologiſts adduce facts to prove the contrary (CCXLIX), I was under the neceſſity of examining the ſtomach of various animals, with this ſole view.

Four hens were fed with kid, and in two hours one was killed: the ſtomach was full; the fleſh ſtill retained its natural ſweet ſavour, which at the ſurface was mixed with a bitteriſh taſte, occaſioned by its being impregnated with the gaſtric fluid. It had no ſmell, except that of this fluid. An hour afterwards the ſtomach of another hen was examined; and here the fleſh was beginning to be converted into a gelatinous paſte, its ſmell was rather diſagreeable; I know not how to deſcribe it, but it was not at all penetrating, or putrid; the colour was ſtill reddiſh, it had not the leaſt nauſeous taſte, nor did it effervefce with acids, or change the colour of ſyrup of violets. Thus we ſee, it ſhewed no ſign either of incipient, or advanced putrefaction. In another hour the third hen was killed: the ſtomach contained a pultaceous maſs, more fluid than in the former caſe; but there was not the ſmalleſt token of putrefaction, any more than in the fourth hen, which was opened three hours afterwards, when the craw was empty, and the contents of the gizzard were now diſſolved.

CCLII. Some frogs juſt killed were ſet before two herons; the birds being hungry, devoured them greedily. In ſix hours one of them was opened; but whether the toughneſs of the ſkin retarded digeſtion, or whether that proceſs is ſlow in herons, the frogs had not loſt their ſhape; though the heads and limbs were either ſeparated, or on the point of being ſeparated from the trunk, and the fleſh was become very ſoft. The taſte, except the uſual bitterneſs, had nothing nauſeous, and the ſmell was by no means putrid. I waited five hours longer before I killed the ſecond, when I found but little fleſh in the ſtomach, and that little was entirely decompoſed, but did not emit the leaſt putrid ſmell.

If fowls and herons afforded no token of putrefaction, much leſs could I expect it from the birds upon which my next trials were made; I mean, young owls, which digeſt fleſh in three, or at moſt four hours. A young dog and cat were next fed, at the ſame time, with boiled beef. The former was opened in four hours and an half. The ſtomach was full of a maſs of ſoftened fleſh, which emitted a very ſlight ſmell, exactly reſembling the ſmell of the gaſtric fluid. The ſtomach of the cat was opened in five hours and an half, and was found to contain ſome remains of fleſh, or rather a pulpy matter, which as in the former caſe, had the ſmell of gaſtric fluid. The fleſh, when nearly digeſted, did not change the colour of ſyrup of violets, or effervefce with acids.

CCLIII. There are animals which retain the food in the ſtomach for a much longer time, as the falcon. Of that upon which I made ſo many experiments, I have already obſerved, that it would devour a whole pigeon at once, and continue without food the whole day afterwards (CLX). Whence, as alſo from its great bulk, we may infer, that the fleſh remains a long time in the body before it is entirely digeſted. Some months after this was killed, another of a different ſpecies fell into my hands; it was of a larger ſize, and had no craw, ſo that the food paſſed immediately into the ſtomach. Notwithſtanding it was pretty tame, and therefore valuable, yet I ſacrificed it for the ſake of theſe experiments, eighteen hours after it had devoured a chicken. What remained in the ſtomach weighed two ounces; it conſiſted of a pulp, in which the fibres could yet be diſcerned; but neither when ſubjected to the before-mentioned chemical trials, or when ſmelled and taſted, did it ſhew any ſign of putrefaction. But among the animals that retain their food for a long time in the ſtomach, thoſe with cold blood, and eſpecially vipers, are, as we have ſeen, the moſt remarkable. A piece of lizard's tail preſerved ſomewhat of its muſcular ſtructure, after having remained five days in the ſtomach of a land ſnake (CXVIII). Three water ſnakes had not conſumed all their food at the end of three days (CXXI). Another not even in ſix days (CXXV). A lizard remained ſixteen days in the ſtomach of a viper, without loſing its natural form (CXXVII). Other cold animals, ſuch as eels, newts, and frogs, muſt not be forgotten. Four eels that had eaten fiſh, retained a little after the expiration of three days, eighteen hours (CXXIX). On the fifth day, ſome frogs had not quite digeſted pieces of inteſtine (CVI); which alſo happened to newts, two days after they had been fed with earth-worms (CVIII). But notwithſtanding the food continued ſo long in the ſtomach of theſe ſeveral animals, I have expreſsly noticed, that it had begun to putrify (CXXVII).

CCLIV. I have met only with two inſtances which do not coincide with this invariable conſtancy of nature, though they do not detract from the certainty of the conſequences that are to be deduced from it. Among the crows that were obliged to ſwallow tubes for a conſiderable length of time, ſome ſuffered in their health, and became poor, though they were copiouſly ſupplied with food. But as they did not take it voluntarily, and as it was my wiſh to keep them alive for the ſake of my experiments, I forced ſome fleſh down the throats of two, but to no purpoſe; for they both died, one thirteen, the other fifteen hours afterwards. My curioſity led me to open them, and I found that the fleſh continued whole and undigeſted, and moreover that it was become putrid. But this evidently aroſe from the morbid condition of the animal, by which the gaſtric fluid was altered, and rendered inefficacious. For this ſpecies of bird, as I have ſeen in a hundred inſtances, digeſts fleſh very ſpeedily, and without any token of putrefaction appearing. It is alſo probable, that the putrid ſtate of the food in the animals mentioned in the CCXLIXth paragraph, aroſe from their morbid condi-

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tion; eſpecially as it remained ſo long in the ſtomach of ſome of them. It may alſo happen, that an animal may die without any diſeaſe preceding, and yet the food be found fœtid, if the ſtomach is opened after a long interval, as frequently happens.

In the ſame paragraph, the breath of the lion and eagle are ſaid to be fœtid. That of the lion I never had an opportunity of examining, but with the eagle it was far otherwiſe; for when I ſtroaked the head gently, it would ſometimes open its mouth, and raiſe a little cry; on theſe occaſions it neceſſarily made a long expiration, and in winter the breath appeared in the form of a little cloud. This cloud I have often ſmelled, and cauſed others to ſmell, both when the bird was faſting, and when the ſtomach was full, and when the food was recently digeſted; but it was never fœtid, and indeed did not ſeem to have any kind of odour.

CCLV. The experiments deſcribed in the CCL, CCLI, CCLII, and CCLIIId paragraphs, not only ſhew that digeſtion is unaccompanied by putrefaction, but might induce us to ſuppoſe, that the ſtomach is provided with an antiſeptic principle. Fleſh incloſed in the pyriform glaſſes that were introduced into the ſtomach of crows, ſhewed clear ſigns of putrefaction in ten hours; whereas in eighteen they ſhew no appearances of the kind, when it is in immediate contact with the ſtomach (CCLIII). And although ſerpents, and the other amphibious animals above-mentioned (CCLIII) are of a cold temperature, yet in their temperature, which is nearly equal to that of the atmoſphere, fleſh becomes putrid in two days, and ſometimes in one, and ſometimes even in a ſhorter time, while in their ſtomachs it remains untainted frequently for a much longer ſpace. I could not therefore but conclude, that there is preſent in theſe caſes, ſome cauſe that prevents the corruption which ſupervenes out of the body. What then is this cauſe? It was not difficult to detect it. I called to mind thoſe unfiniſhed digeſtions, which take place when fleſh is immerſed in gaſtric fluid contained in phials. Where it is diſſolved without ever turning putrid, notwithſtanding it is kept long enough, and expoſed to a ſufficient heat. I could not then doubt, that the gaſtric juices are at once the ſolvent, and the preſervative from putrefaction. Further reflection furniſhed me with proofs ſtill more deciſive. It appears from various paſſages in the preceding diſſertations, that in attempts to produce artificial digeſtion, little or no ſolution takes place, unleſs the fluid extracted from the ſtomach is expoſed to a briſk heat (CXLII, CLXXXVI, CCI, CCXVII). But without this condition, it retains its antiſeptic powers (CLXXXVI, CCXVII). Two phials, containing one ſome gaſtric fluid from a crow, and the other from a dog, together with ſome veal and mutton, were kept thirty-ſeven days in winter, in an apartment without fire: the fleſh was not either conſumed or turned putrid; while ſome that was immerſed in water, began to emit a fœtid ſmell on the ſeventh, and about the thirtieth day was changed into a very offenſive liquamen. It is proper to add, that the gaſtric fluid at laſt loſes, though kept in phials ever ſo cloſely ſtopped, its antiſeptic quality, but it never becomes putrid itſelf. This at leaſt I found to be the caſe with ſome taken from a crow, after it had been kept two months.

CCLVI. The diſcovery of this antiſeptic property led me to enquire, what would be the effect of immerſing fleſh, more or leſs putrid, in gaſtric fluid. Four portions that had an inſupportable ſmell, were ſet in four bottles, which I filled with four different kinds of gaſtric fluid, viz. of a dog, a crow, an owl, and an eagle. This was done in March, and the bottles were kept twenty-five days in an apartment, where the heat was never leſs than 8, and never exceeded 12°. I could not perceive that it was at all more diſſolved than if it had been immerſed in water. With reſpect to the fœtid ſmell in the phials containing lamb and veal, it continued unchanged; but in the two others, which contained fowl and pigeon, it ſeemed rather diminiſhed. This reſult ſuggeſted to me, that the gaſtric fluid might not only impede putrefaction, but reſtore putrified ſubſtances. I therefore repeated the experiment in June, and found that my ſuſpicion was well founded. Some fowl and pigeon, in which putrefaction was pretty far advanced, were immerſed in the gaſtric fluid of a dog and falcon, and remained in it thirty-ſeven hours, in which time they were reduced to a jelly, but had loſt moſt of their diſguſting ſmell. On comparing this with the preceding experiment, I conjectured, that the ſuperior efficacy of the gaſtric fluid in the latter caſe, proceeded from the warmth of the ſeaſon: and this induced me to expoſe the ſame fleſh under the ſame circumſtances, to the ſun about the middle of June. And now ten hours completely took away the fœtid ſmell. I did not neglect making the ſame experiment with the gaſtric fluid of other animals; the fleſh generally loſt its diſagreeable odour, but ſometimes for a reaſon, which I cannot aſſign, retained it in part. It is proper to add, that the recent fluid was always more efficacious than the old.

CCLVII. If we conſider the CCLV and CCLVIth paragraphs, we muſt conclude, that putrid fleſh loſes this quality in the ſtomach of animals. Before I attempted to aſcertain this point by experiment, nature herſelf gave me a deciſive proof of it. At the time I kept a great number of fowls for my enquiries, I perceived that when they are allowed to eat at will, they cram their craw ſo full, that it is ſixteen or twenty hours before it is completely evacuated. Curioſity led me to kill a cockrel that had about an ounce of meat, which happened to be bruiſed fleſh, remaining in its craw: and I was ſtruck with ſurprize, when I perceived that it had a ſtrong putrid ſmell: it was become ſoft, had a dull red colour, and a nauſeous taſte: I immediately proceeded to examine the contents of the ſtomach; but here I found the fleſh quite decompoſed with a bitter ſweet taſte, and a ſmell by no means fœtid. The liquor therefore of the ſtomach, had corrected the putrid quality which the fleſh had acquired in the craw. The ſame thing took place in ſome hens. The fleſh in the craw became putrid in ſixteen hours, while that in the ſtomach had no diſagreeable odour. It ſhould; however, be remarked, that the putrefactive fermentation never runs ſo high within the craw, as it does out of the body, even when the heat is leſs ſtrong. Whence I ſuſpected, that the fluid of the craw might alſo poſſeſs an antiſeptic power, though in a degree very far inferior to that of the ſtomach.

CCLVIII. I took a putrid piece of beef's lights, and dividing it into five portions, faſtened a ſtring to each, and then thruſt them into the ſtomachs of five ravens. The end of the ſtring was brought out at the beak, as on former occaſions (LXVIII), that I might be able to examine the fleſh at pleaſure. In three quarters of an hour, two of the pieces were drawn up: they were waſted, and at firſt ſeemed to have loſt their putrid ſmell, but upon wiping off the gaſtric fluid, it became again ſenſible, but it was much diminiſhed. Half an hour afterwards another piece, upon examination, was found to be ſtill more waſted, and to have loſt almoſt all its bad odour, even when the gaſtric fluid was carefully wiped away. In an hour afterwards the two remaining pieces were drawn up. They were reduced to the ſize of a pea, and it would have been impoſſible to tell that they had been ever putrid, ſo perfectly were they recovered; even the taſte had nothing diſguſting, except the bitterneſs which is always preſent on ſuch occaſions.

The great length of the neck prevented me from repeating this experiment upon the heron. I forced a ſemi-putrid frog, from which the ſkin had been taken, into the ſtomach, but I could not draw it up again; it was therefore neceſſary to cut the ſtring at the beak, when the bird immediately ſwallowed it. It was my intention to kill the heron in about an hour, that I might examine what change the frog had undergone. But it was vomited before that time, probably on account of its being a diſguſting food; for however greedily the heron devours living fiſhes and frogs, it abſtains from them when they are turning putrid. The gaſtric fluid had, notwithſtanding, exerted both its antiſeptic and ſolvent powers, during the forty-three minutes the frog had been in the ſtomach. Some tin tubes were then filled with putrid fiſh, and given to the animal; they were not, as before, thrown up, perhaps, becauſe the putrid matter was not in contact with the ſtomach. The bird was killed three hours afterwards; what remained in the tubes weighed one-ſeventh of an ounce; it reſembled a thick gelatinous paſte, in which a few fleſhy fibres might yet be diſtinguiſhed, and which retained no veſtige of its former putrid ſtate.

CCLIX. I treated ſeveral ſmall birds of prey, ſuch as the two ſpecies of owl above deſcribed, and a young hawk, as I had done crows (CLVIII). They were fed with inteſtine, liver, and lungs of ſheep, more or leſs putrid. Solution took place, and the putrefaction was corrected according to the time of the continuance of the fleſh in the ſtomach. The hawk twice threw up what it had ſwallowed, probably, becauſe the putrid ſtate of it diſagreed with the ſtomach, for this never happened when it was fed with freſh meat. The gaſtric juices of the eagle produced the ſame effect upon fleſh incloſed in tubes, and introduced into its ſtomach. Animals of cold blood having very ſlow digeſtive powers, were long in correcting the putrefaction of fleſh. This effect, however, was at laſt produced. The only precaution neceſſary, was to return the ſubſtances into the ſtomach when they were vomited, which often happened.

The laſt experiments I made with this view, were upon a cat, a dog, and myſelf. I was obliged to force the putrid fleſh down the throat of theſe animals, for notwithſtanding they were exceedingly hungry, they obſtinately refuſed it. The dog retained what was forced upon him, but the cat vomited it along with a quantity of foam, and a liquor that appeared to be gaſtric fluid. The fleſh, which when it was given was exceedingly fœtid, had now loſt its ſmell entirely; of this, another cat, by eating it without afterwards throwing it up, gave me a clear proof. Upon opening the ſtomach, I found the fleſh half digeſted, and with no other ſmell than that which freſh meat uſually emits in like circumſtances. In two hours and a half the dog was opened. The fleſh lay in a little lake of gaſtric fluid, nearly decompoſed, nor did it either in taſte or ſmell reſemble tainted meat. The experiment I made on myſelf, conſiſted in ſwallowing, at five different times, five tubes covered with linen, like thoſe mentioned in the CCVIIIth paragragh: they were full of different ſorts of putrid fleſh. I voided them ſeparately, and in each there was ſome of the contents remaining, but not one exhibited the ſmalleſt token of putrefaction. Hence then it appears, that the various claſſes of animals, and Man among the reſt, in an healthy ſtate, are endowed with the power, not only of checking the putrefaction of ſubſtances lodged in the ſtomach, but alſo of correcting them when already putrid.

CCLX. By this diſcovery I was led to reflect, that many animals living upon fleſh, and matters that have a tendency to run into the putrefactive fermentation, never feed but upon ſuch as are freſh and ſweet; and that, if by any accident putrid food ſhould get into the ſtomach, they are ſubject to vomiting and various bad ſymptoms, and even death itſelf: ſome inſtances of vomiting excited by this cauſe, may be ſeen above (CCLVIII, CCLIX); while on the other hand, many animals delight in corrupted ſubſtances, as the multitude of loathſome inſects and worms that reſide in ſewers and ſepulchres, and feed upon decaying carcaſes. Among birds and quadrupeds, there are alſo ſome that ſeek tainted fleſh; ſuch are the crow, the kite, the vulture, among the former; and among the latter, the chacal and the hyena. While other animals fly the miaſmata that ariſe from bodies in ſuch a ſtate, theſe ſeek and are guided by them to their abominable repaſts. But now we are acquainted with the antiſeptic virtue of the gaſtric fluid, the diſguſting manners of theſe animals ought no longer to ſurprize us, for the food, however putrid, muſt be totally changed before it is converted into nutriment and animalized. And although the putrid quality is corrected by other animals, yet food in that ſtate is noxious to them, on account of the diſagreeable impreſſion it makes on the organs of ſmell and taſte, as alſo upon the ſtomach, by which, and particularly by their noiſome miaſmata, the nervous ſyſtem is probably irritated. It beſides ſeems likely, that the antiſeptic power of the gaſtric fluid of the former, is greater and more efficacious, and conſequently that it more readily and more completely corrects putrefaction. Habit, which is juſtly reputed a ſecond nature, may bring animals, that naturally abominate putrid food, to live very well upon it. We have already witneſſed the converſion of a pigeon from a granivorous into a carnivorous animal (CLXXV); and I brought it to eat not only freſh fleſh, but ſuch as was fœtid, and even completely putrified. The bird at firſt abſolutely refuſed it, and I was obliged to force it into the ſtomach; for ſome days it ſuffered from this treatment, and became evidently leaner. But by degrees nature became inured to the food, and the pigeon, ſtimulated by hunger, took it ſpontaneouſly, till at laſt it recovered its plumpneſs; and now its appetite for tainted, was as keen as it had been before for ſweet meat. We may learn from this inſtance, that cuſtom is capable of changing diſagreeable, and even noxious food, into good nouriſhment.

CCLXI. But what ſhall we ſuppoſe enables the gaſtric fluid to check and correct putrefaction? As it contains a ſalt, and that of the ammonical kind (CCXLVI), and as beſides the experiments of Pringle ſhew, that all ſalts, whether acid, alkaline, or neutral, whether volatile or fixed, are antiſeptic (*a*), it is ob-

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(*a*) Appendix, containing experiments on ſeptic and antiſeptic ſubſtances.

obvious to conjecture, that theſe two qualities ariſe from the ſame ſource. I conceived, however, before I determined abſolutely, that it would be proper to attempt a few experiments. It is obſerved by Pringle, that we muſt employ common ſalt, which ſo nearly reſembles ſal ammoniac, in conſiderable quantity, if we wiſh it to act as an antiſeptic; otherwiſe, it is ſo far from checking, that it promotes putrefaction. Thus a drachm of ſalt, diſſolved in two ounces of water, keeps meat ſweet but a little while, and twenty-five grains a ſtill leſs time; while ten, fifteen, and even twenty grains haſten its corruption. This paradox has been confirmed in France by the learned Mr. Gardane. Notwithſtanding theſe authorities, I determined to bring it to the teſt of experiment. I therefore took four phials, and putting into each three pennyweights, ſix grains of freſh beef, pounded very ſmall, I poured upon it an ounce and half of water. In the firſt phial were diſſolved ten grains of common ſalt, in the ſecond fifteen, in the third twenty, and the fourth was left without ſalt, as a term of compariſon. The temperature of the place where they were kept, was about fifteen degrees. The firſt phial began firſt to emit a fœtid ſmell, the fourth next, then the ſecond, and laſtly the third. The other tokens of putrefaction appeared in the ſame order. When ſal ammoniac was ſubſtituted in the place of common ſalt, the only difference in the reſult, conſiſted in the phial which contained no ſalt, and that which contained ten grains, beginning to exhale a putrid ſmell at the ſame time. It appears, therefore, that Pringle's experiment was accurate, and that the ſame thing nearly is true of ſal ammoniac. In order to determine whether the antiſeptic property of the gaſtric fluid ariſes from the ſal ammoniac it contains, I diſſolved a quantity of that ſalt by degrees in water, till it had nearly acquired the ſame ſaltneſs as the gaſtric fluid; ſome bruiſed fleſh was then immerſed in it. That the water and the liquor of the ſtomach had nearly the ſame ſaltneſs, I aſſured myſelf, both by taſting it, and by dropping a few drops of each into a ſolution of ſilver in the nitrous acid, when each afforded the ſame white precipitate. But it is this cauſe that prevents putrefaction; for the fleſh immerſed in the ſalt water, emitted a fœtid odour ſooner than other fleſh of the ſame kind, infuſed in common water: and although when more ſal ammoniac was employed, putrefaction was retarded, it was not prevented; to attain this end, eighteen or twenty times as much ſalt as is contained in the gaſtric fluid was requiſite. Theſe facts ſeem clearly to ſhew, that the antiſeptic quality of the gaſtric fluid does not depend on the ſmall quantity of ſal ammoniac it contains.

CCLXII. From the ſeptic power of common ſalt in ſmall quantity, Mr. Gardane deduces a conſequence, which it may be proper to notice in paſſing. He thinks the common ſalt we take with our food, being always in little doſes, forwards digeſtion, by promoting putrefaction; upon which, according to him, as we have ſeen above (CCXLIX), that function depends. Though my numerous experiments completely deſtroy this ſuppoſition, yet it ſeemed worth while to try what would happen to fleſh ſeaſoned with ſuch a proportion of common ſalt as haſtens putrefaction, and given to different animals. Some tubes, filled with fleſh thus prepared, and others, with ſome of the ſame kind, without ſalt, were given to a dog and a cat. The animals were opened in five hours, and upon examining the tubes, I could not perceive that the ſalt had occaſioned any difference. What remained undiſſolved, had ſtill a ſlight ſalt taſte, but not the leaſt diſagreeable ſmell; and it was juſt as much waſted as the other. It therefore appears, that this ſmall doſe of ſalt had neither promoted digeſtion, nor produced any tendency to putrefaction, being overpowered by the antiſeptic quality of the gaſtric fluid.

CCLXIII. But to return from this digreſſion. If the ſalt contained in the gaſtric fluid is not the cauſe of its antiſeptic power, to what other principle can it be owing? Macbride's theory concerning the origin of this property in ſo many bodies, has great ingenuity. The coheſion and ſolidity of ſubſtances, is in his opinion owing to the fixed air they contain. Now when by any means this is taken away, the mutual adheſion of the ſeveral parts will be deſtroyed, and the body will either run into the putrefactive fermentation, or crumble into duſt, according to the nature of its conſtituent parts. Hence it neceſſarily follows, that whatever ſubſtance has the power of impeding the ſeparation of fixed air, or reſtoring it when ſeparated, will alſo prevent or correct putrefaction. But antiſeptic matters have, according to this phyſician, ſuch a power. A piece of fleſh, for inſtance, ſurrounded by a ſubſtance of this kind, is kept ſweet, becauſe the fixed air cannot make its eſcape; and that, probably, on account of its pores being blocked up by the finer particles of the antiſeptic matter. Hence the fleſh will long preſerve its natural taſte and conſiſtence. If it has already become putrid, it will receive fixed air from the antiſeptic body, and hence ceaſe by degrees to exhale a fœtid ſmell, loſe its fluidity and flabbineſs, and at laſt recover its ſweetneſs and firmneſs (*a*).

Will not this theory account for the antiſeptic power of the gaſtric fluid? Without going out of my way to examine the foundation on which it reſts, I will obſerve, that it ſeems by no means to afford the information wanted, ſince the gaſtric fluid is an antiſeptic of a ſingular ſort. Other ſubſtances poſſeſſing this property, while they keep away putrefaction, preſerve or reſtore the coheſion of the parts; whereas the gaſtric fluid being at once an antiſeptic and ſolvent, while it prevents or corrects putrefaction, reduces bodies into very ſmall particles. We muſt therefore conclude, that the property of this animal fluid ariſes from ſome other principle, though I cannot determine what that principle is, both for want of experimental data, and on account of the imperfect ſtate in which phyſicians have left the theory of putrefaction. I therefore choſe to acknowledge my ignorance, rather than invent ſome gratuitous hypotheſis; ſuch a mode of proceeding would ill agree with the diſpoſition of one, who has

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(*a*) Macbride, l. c.

no other object in view than the diſcovery of truth.

CCLXIV. For the ſake of my readers, it may be proper to recapitulate what has been proved in this diſſertation. Firſt, of the three ſpecies of fermentation eſtabliſhed by modern chemiſts and naturaliſts, viz. the ſweet, the acetous, and the putrid, neither takes place in digeſtion. Secondly, Though this function is ſometimes accompanied with an acid, yet this principle diſappears entirely towards the concluſion of it. Thirdly, Putrefaction never in health attends digeſtion. Fourthly, The gaſtric fluid is a real antiſeptic. I ſuppoſe my proofs, however concluſive, will not avail with thoſe who eſtabliſh it as an axiom, that wherever there is heat and moiſture, there muſt be fermentation; and think that it muſt therefore neceſſarily take place in the food, and not only in the ſtomach and inteſtines, but in the chyliferous and ſanguiferous veſſels: they indeed limit their doctrine ſo far as to ſay, that whereas out of the body it goes on rapidly, and with an inteſtine commotion, in the body of it is ſlow, weak, and generally imperceptible. Let me intreat theſe learned and zealous advocates for fermentation to reflect, that my experiments are not directly repugnant to theirs. I only pretend to ſhew, that not the ſmalleſt ſenſible fermentation takes place in the ſtomach of animals or man. With reſpect to inſenſible fermentation, as it is amongſt uncertain things, ſound logic forbids me alike either to admit or reject it.

APPENDIX.

ON THE DIGESTION OF THE STOMACH AFTER DEATH.

By JOHN HUNTER, F. R. S. and Surgeon to ST. GEORGE's Hoſpital *.

AN accurate knowledge of the appearances in animal bodies that die of a violent death, that is, in perfect health, or in a ſound ſtate, ought to be conſidered as a neceſſary foundation for judging of the ſtate of the body in thoſe that are diſeaſed.

But as an animal body undergoes changes after death, or when dead, it has never been ſufficiently conſidered what thoſe changes are; and till this be done, it is impoſſible we ſhould judge accurately of the appearances in dead bodies. The diſeaſes which the living body undergoes (mortification excepted) are always connected with the living principle, and are not in the leaſt ſimilar to what may be called diſeaſes or changes in the dead body:

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* See Philoſophical Tranſactions, Vol. LXII. p. 447.

without this knowledge, our judgment of the appearances in dead bodies muſt often be very imperfect, or very erroneous; we may ſee appearances which are natural, and may ſuppoſe them to have ariſen from diſeaſe; we may ſee diſeaſed parts, and ſuppoſe them in a natural ſtate; and we may ſuppoſe a circumſtance to have exiſted before death, which was really a conſequence of it; or we may imagine it to be a natural change after death, when it was truly a diſeaſe of the living body. It is eaſy to ſee therefore, how a man in this ſtate of ignorance muſt blunder, when he comes to connect the appearances in a dead body with the ſymptoms that were obſerved in life; and indeed, all the uſefulneſs of opening dead bodies depends upon the judgment and ſagacity with which this ſort of compariſon is made.

There is a caſe of a mixed nature, which cannot be reckoned a proceſs of the living body, nor of the dead; it participates of both, inaſmuch as its cauſe ariſes from the living, yet cannot take effect till after death.

This ſhall be the object of the preſent paper; and, to render the ſubject more intelligible, it will be neceſſary to give ſome general ideas concerning the cauſe and effects.

An animal ſubſtance, when joined with the living principle, cannot undergo any change in its properties but as an animal; this principle always acting and preſerving the ſubſtance, which it inhabits, from diſſolution, and from being changed according to the natural changes, which other ſubſtances, applied to it, undergo.

There are a great many powers in nature, which the living principle does not enable the animal matter, with which it is combined, to reſiſt, viz. the mechanical and moſt of the ſtronger chemical ſolvents. It renders it however capable of reſiſting the powers of fermentation, digeſtion, and perhaps ſeveral others, which are well known to act on this ſame matter, when deprived of the living principle, and entirely to decompoſe it. The number of powers, which thus act differently on the living and dead animal ſubſtance, is not aſcertained: we ſhall take notice of two, which can only affect this ſubſtance when deprived of the living principle; which are, putrefaction and digeſtion. Putrefaction is an effect which ariſes ſpontaneouſly; digeſtion is an effect of another principle acting upon it, and ſhall here be conſidered a little more particularly.

Animals, or parts of animals, poſſeſſed of the living principle, when taken into the ſtomach, are not the leaſt affected by the powers of that viſcus, ſo long as the animal principle remains; hence it is that we find animals of various kinds living in the ſtomach, or even hatched and bred there: but the moment that any of thoſe loſe the living principle, they become ſubject to the digeſtive powers of the ſtomach. If it were poſſible for a man's hand, for example, to be introduced into the ſtomach of a living animal, and kept there for ſome conſiderable time, it would be found, that the diſſolvent powers of the ſtomach could have no effect upon it; but if the ſame hand were ſeparated from the body, and introduced into the ſame ſtomach, we ſhould then find that the ſtomach would immediately act upon it.

Indeed, if this were not the caſe, we ſhould find that the ſtomach itſelf ought to have been made of indigeſtible materials; for, if the living principle was not capable of preſerving animal ſubſtances from undergoing that proceſs, the ſtomach itſelf would be digeſted.

But we find on the contrary, that the ſtomach, which at one inſtant, that is, while poſſeſſed of the living principle, was capable of reſiſting the digeſtive powers which it contained, the next moment, viz. when deprived of the living principle, is itſelf capable of being digeſted, either by the digeſtive powers of other ſtomachs, or by the remains of that power which it had of digeſting other things.

From theſe obſervations, we are led to account for an appearance which we often find in the ſtomachs of dead bodies; and at the ſame time they throw a conſiderable light upon the nature of digeſtion. The appearance which has been hinted at, is a diſſolution of the ſtomach at its greateſt extremity; in conſequence of which, there is frequently a conſiderable aperture made in that viſcus. The edges of this opening appear to be half diſſolved, very much like that kind of diſſolution which fleſhy parts undergo when half digeſted in a living ſtomach, or when diſſolved by a cauſtic alkali, viz. pulpy, tender, and ragged.

In theſe caſes, the contents of the ſtomach are generally found looſe in the cavity of the abdomen, about the ſpleen and diaphragm. In many ſubjects this digeſtive power extends

much

much further than through the ſtomach. I have often found, that after it had diſſolved the ſtomach at the uſual place, the contents of the ſtomach had come into contact with the ſpleen and diaphragm, had partly diſſolved the adjacent ſide of the ſpleen, and had diſſolved the diaphragm quite through; ſo that the contents of the ſtomach were found in the cavity of the thorax, and had even affected the lungs in a ſmall degree.

There are very few dead bodies, in which the ſtomach is not, at its great end, in ſome degree digeſted; and one who is acquainted with diſſections, can eaſily trace the gradations from the ſmalleſt to the greateſt.

To be ſenſible of this effect, nothing more is neceſſary, than to compare the inner ſurface of the great end of the ſtomach, with any other part of the inner ſurface; what is found, will appear ſoft, ſpongy, and granulated, and without diſtinct blood-veſſels, opake and thick; while the other will appear ſmooth, thin, and more tranſparent; and the veſſels will be ſeen ramifying in its ſubſtance, and upon ſqueezing the blood which they contain from the larger branches to the ſmaller, it will be found to paſs out at the digeſted ends of the veſſels, and appear like drops on the inner ſurface.

Theſe appearances I had often ſeen, and I do ſuppoſe that they had been ſeen by others; but I was at a loſs to account for them; at firſt, I ſuppoſed them to have been produced during life, and was therefore diſpoſed to look upon them as the cauſe of death; but I never found that they had any connection with the ſymptoms: and I was ſtill more at a loſs to account for theſe appearances, when I found that they were moſt frequent in thoſe who died of violent deaths, which made me ſuſpect that the true cauſe was not even imagined (*a*).

At this time I was making many experiments upon digeſtion, on different animals, all of which were killed, at different times, after being fed with different kinds of food; ſome of them were not opened immediately after death, and in ſome of them I found the appearances above deſcribed in the ſtomach. For, purſuing the enquiry about digeſtion, I got the ſtomachs of a vaſt variety of fiſh, which all die of violent deaths, and all may be ſaid to die in perfect health, and with their

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(*a*) The firſt time that I had occaſion to obſerve this appearance in ſuch as died of violence and ſuddenly, and in whom therefore I could not eaſily ſuppoſe it to be the effect of diſeaſe in the living body, was in a man who had his ſkull fractured, and was killed outright by one blow of a poker. Juſt before this accident, he had been in perfect health, and had taken a hearty ſupper of cold meat, cheeſe, bread, and ale. Upon opening the abdomen, I found that the ſtomach, though it ſtill contained a good deal, was diſſolved at its great end, and a conſiderable part of theſe its contents lay looſe in the general cavity of the belly. This appearance puzzled me very much. The ſecond time was at St. George's Hoſpital, in a man who died a few hours after receiving a blow on his head, which fractured his ſkull likewiſe. From thoſe two caſes, among other conjectures about ſo ſtrange an appearance, I began to ſuſpect that it might be peculiar to caſes of fractured ſkulls; and therefore, whenever I had an opportunity, I examined the ſtomach of every perſon who died of that accident: but I found many of them which had not this appearance. Afterwards I met with it in a ſoldier who had been hanged.

ſtomach commonly full; in theſe animals we ſee the progreſs of digeſtion moſt diſtinctly; for as they ſwallowed their food whole, that is, without maſtication, and ſwallow fiſh that are much larger than the digeſting part of the ſtomach can contain (the ſhape of the fiſh ſwallowed being very favourable for this enquiry), we find in many inſtances that the part of the ſwallowed fiſh which is lodged in the digeſting part of the ſtomach is more or leſs diſſolved, while that part which remains in the œſophagus is perfectly ſound.

And in many of theſe I found, that this digeſting part of the ſtomach was itſelf reduced to the ſame diſſolved ſtate as the digeſted part of the food.

Being employed upon this ſubject, and therefore enabled to account more readily for appearances which had any connection with it, and obſerving that the half-diſſolved parts of the ſtomach, &c. were ſimilar to the half-digeſted food, it immediately ſtruck me, that it was from the proceſs of digeſtion going on after death, that the ſtomach, being dead, was no longer capable of reſiſting the powers of that menſtruum, which itſelf had formed for the digeſtion of its contents; with this idea, I ſet about making experiments to produce theſe appearances at pleaſure, which would have taught us how long the animal ought to live after feeding, and how long it ſhould remain after death before it is opened; and above all, to find out the method of producing the greateſt digeſtive power in the living ſtomach: but this purſuit led me into an unbounded field.

Theſe appearances throw conſiderable light on the principles of digeſtion; they ſhew that it is not mechanical power, nor contractions of the ſtomach, nor heat, but ſomething ſecreted in the coats of the ſtomach, which is thrown into its cavity, and there animaliſes the food (*a*), or aſſimilates it to the nature of the blood. The power of this juice is confined or limited to certain ſubſtances, eſpecially of the vegetable and animal kingdoms; and although this menſtruum is capable of acting independently of the ſtomach, yet it is obliged to that viſcus for its continuance.

(*a*) In all the animals, whether carnivorous or not, upon which I made obſervations or experiments to diſcover whether or not there was an acid in the ſtomach, (and I tried this in a great variety), I conſtantly found that there was an acid, but not a ſtrong one, in the juices contained in that viſcus in a natural ſtate.

EXPERIMENTS
CONCERNING
DIGESTION.
TRANSLATED FROM THE
INAUGURAL DISSERTATION
OF
DR. STEVENS.

Publiſhed at Edinburgh in 1777.

THE following experiments were made at Edinburgh upon an Huſſar, a man of weak underſtanding, who gained a miſerable livelihood, by ſwallowing ſtones for the amuſement of the common people, at the imminent hazard of his life. He began this practice at the age of ſeven, and has now followed it twenty years. His ſtomach is ſo much diſtended, that he can ſwallow ſeveral ſtones at a time; and theſe may not only be plainly felt, but may be heard, whenever the hypogaſtric region is ſtruck.

EXPERIMENT I.

At eight o'clock in the evening, I gave the ſubject of my experiments a hollow ſilver ſphere, divided into two cavities by a partition, and perforated on the ſurface with a great number of holes, capable of admitting a needle: into one of theſe cavities was put four ſcruples and a half of raw beef, and into the other five ſcruples of raw bleak. The ſphere was voided in twenty-one hours, when the beef was found to have loſt one ſcruple and a half, and the fiſh two ſcruples. The reſt was much ſoftened, but had no diſagreeable ſmell.

II. A few days afterwards he took the ſame ſphere, containing in one cavity a ſcruple and four grains of raw beef, and in the other four ſcruples and eight grains of the ſame boiled. In forty-three hours the ſphere was returned, and the raw fleſh had loſt one ſcruple and two grains, and the boiled one ſcruple and ſixteen grains.

III. Suſpecting that if theſe ſubſtances were divided, ſo that the ſolvent could have freer acceſs to them, more of them would be diſſolved, I procured another ſphere with holes, ſo large as to receive a crow's quill, and encloſed ſome beef a little maſticated in it. It was voided quite empty, thirty-eight hours after it was ſwallowed.

IV. Seeing how readily the chewed meat was diſſolved, I thought of trying whether it would be as ſoon diſſolved in a ſphere with large holes, but without being chewed. I therefore put a ſcruple and eight grains of pork into one cavity, and into the other the ſame quantity of cheeſe. The ſphere was retained forty-three hours, at the end of which not the ſmalleſt remains of either pork or cheeſe could be found.

V. He afterwards ſwallowed the ſame ſphere, containing in one partition ſome roaſted turkey, and in the other ſome boiled ſalt herring. In forty-ſix hours it was voided, and nothing of the turkey or herring now appeared, both having been completely diſſolved.

VI. Having found that animal ſubſtances, though incloſed in tubes, are eaſily concocted, I next determined to try whether vegetables, which are more difficultly digeſted, would be ſo too. I therefore encloſed an equal quantity of raw parſnep and potatoe in a ſphere. It was voided after having continued forty-eight hours in the alimentary canal, when both ſpecies of vegetable were found to be diſſolved.

VII. Pieces of apple and turnep, both raw and boiled, were diſſolved in thirty-ſix hours.

VIII. He next ſwallowed ſome grains of wheat, rye, barley, oats, and peaſe, contained in a ſphere, which remained ſeveral hours in the alimentary canal, but no alteration was produced on any of its contents, except upon the peaſe, which were ſwoln, and burſt by the humidity they had imbibed.

IX. The readineſs with which the gaſtric fluid had acted upon roaſted animal ſubſtances, induced me to try what change would be produced by it upon hard ones, ſuch as bone. I therefore incloſed in one partition of a ſphere, ſome of the bone from a leg of mutton,

ton, and in the other part of a turkey's wing. The ſphere was retained forty-eight hours. The bone was weighed, and found to have loſt nothing of its weight, while the fleſh, ſkin, and ligaments were quite diſſolved, ſo that the bones of the wing were now quite ſeparate; but they had undergone no perceptible alteration.

X. Inanimate matters being ſo readily ſoluble, I reſolved to enquire how far living animals are capable of reſiſting the action of this powerful menſtruum. With this view, an animal ſuppoſed to be deſtitute of pores, and, according to my experiments, capable of ſuſtaining a degree of heat equal to the human temperature, was encloſed in a ſphere perforated with ſmall holes, to prevent the leech from wounding the ſtomach. The Huſſar took it, and voided it about the uſual time, when nothing was found except a black viſcid miaſma, the remains of the digeſted leech. This experiment was repeated with earth-worms, and they were diſſolved with equal facility. But as they cannot ſo well ſupport the human temperature, it is probable they died before they began to be diſſolved *.

It was my intention to make more experiments of this kind, but as the Huſſar left Edinburgh ſoon afterwards, I was obliged to have recourſe to dogs and ruminating animals.

XI. A whelp, three months old, having been kept fourteen hours without food, was forced to ſwallow four oval ivory globes, of

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* Perhaps this is alſo the caſe with leeches.

different ſizes, and perforated with many ſmall holes. One contained beef, another haddock, a third potatoe, and a fourth cabbage, all raw, and weighing each ſixteen grains. In four hours the animal was killed and opened. The globes were found in the ſtomach, and their reſpective contents were diminiſhed in the following proportions: The fiſh had loſt nine grains, the beef five, the potatoe three, and the cabbage one. The globes themſelves appeared to be thinner, but as I had no ſuſpicion that the ivory would be affected by the gaſtric fluid, I did not weigh them before the experiment. I could not therefore exactly aſcertain their diminution.

XII. Having procured a whelp five months old, it was kept faſting ſixteen hours, and then four of the globes uſed in the foregoing experiment, each containing a certain quantity of mutton, turbot, parſnep, and potatoe were forced upon it. Theſe ſubſtances had been previouſly expoſed to the action of fire, and each weighed ſixteen grains. Seven hours afterwards the animal was killed, and the globes were taken out of the ſtomach; when the fiſh was found to have loſt ten grains and a half, the mutton ſix, the potatoe five, and the parſnep nothing. The ſpheres were become ſtill thinner, but I had as before, neglected to weigh them.

XIII. A dog ſix months old was kept faſting the uſual time, and the ſame four ſpheres were given him. The firſt contained ſixteen grains of boiled mutton, the ſecond as much boiled fiſh, the third the ſame quantity of boiled potatoe, and the fourth of boiled parſnep. In eight hours it was killed and opened.

ed. The globes were found greatly altered (*a*). The extreme parts, not the middle, were totally diſſolved, ſo that the contents lay looſe in the ſtomach. The ſpheres, before the experiment, weighed together three ſcruples ſixteen grains; the fragments weighed only one ſcruple and twenty grains. The mutton and fiſh were entirely concocted, the potatoe had loſt twenty-one grains; but the parſnep was unchanged.

XIV. Being ſurprized at the ſpeedy ſolution of ivory by the gaſtric fluid, I determined to ſubject other hard bodies to its action. I therefore carefully weighed three pieces of a ſheep's thigh bone, and gave them to a dog that had been long kept faſting. Seven hours afterwards the animal was killed, and the bones were taken out of the ſtomach. The firſt had loſt ſeven, the ſecond nine, and the third twelve grains. The ſolution began at the internal ſurface, and advanced towards the center, ſo that the cavity was conſiderably augmented (*b*).

(*a*) The author has expreſſed this very obſcurely. Partes eorum extremæ, non mediæ, ex toto ſolutæ ſunt, &c. I confeſs I am yet to learn, what are the extreme parts or ends of a ſphere. T.

(*b*) In order to aſſure myſelf that this ſolution was not owing to fermentation, or an acid, I immerſed a bone of the ſame kind, in an alimentary mixture, conſiſting of roaſted beef, wheaten bread and water, beaten into a pulp. When it had remained forty-eight hours in a temperature, equal to 102 deg. of Fahr. Therm. it was examined: the fermentation had run very high, and the acidity was ſtrong, but the bone had undergone no diminution. It was, however, much ſoftened.

I moreover obliged my dog to ſwallow pieces of cartilage, but I found that the gaſtric fluid produced no effect upon them.

XV. As the ivory ſpheres and bones were ſo readily diſſolved in the foregoing experiments, I was induced to make trial of ſome bodies ſtill harder. With this view I procured ſome cylindrical tin tubes, perforated with a great number of holes; of which four were given to a dog that had been kept faſting twelve hours. The firſt contained ſixteen grains of roaſted beef, the ſecond the ſame quantity of veal, the third of fat, and the fourth of wheaten bread. In ten hours the animal was killed and opened, and the tubes were taken out of its ſtomach. The beef and bread were quite diſſolved; the veal had loſt only ten grains, and the fat eight and a half. The tubes had not undergone the ſmalleſt alteration.

XVI. As in the laſt experiment the veal was not ſo ſoon diſſolved as the beef, I began to ſuſpect that the fleſh of young animals in general is leſs eaſy to digeſt than that of old ones. I therefore took care to repeat the experiment with lamb and mutton, which were put in equal quantities into two tubes. The reſult was as before. In ſeven hours the mutton was quite diſſolved, whereas the lamb had loſt only ten grains. The remains of veal and lamb in theſe experiments were ſurrounded with a viſcid gelatinous matter.

XVII. Sixteen grains of raw beef, and the ſame quantity of roaſted were incloſed in two tubes, and given to a dog, which was killed ſeven hours afterwards, when the former was found to have loſt fifteen grains, while the latter was completely diſſolved.

XVIII. The ſame experiment was repeated with fiſh inſtead of fleſh. Sixteen grains of raw and as much boiled haddock, were incloſed in two tubes, and given to a dog. When he was killed, no remains of the boiled could be found; the raw portion had loſt fourteen grains.

XIX. I next enquired whether quadrupeds or birds are moſt eaſily digeſted. For this purpoſe, equal quantities of beef, mutton, and fowl were incloſed in three tubes, and given to a dog; they were each roaſted, and weighed ſixteen grains. Upon killing the dog, and examining the tubes, I found that the mutton and beef had been diſſolved, while the fowl had only loſt eleven grains.

Moſt of the experiments related above, were repeated oftener than once, and afforded the ſame reſult. We cannot therefore entertain any doubt concerning the mode of digeſtion in this claſs of animals. Whether the concoction of ruminating animals is effected in the ſame manner, I endeavoured to aſcertain by the following experiments.

XX. I gave a ſheep four cylindrical tin tubes, each containing ſixteen grains of raw beef, ſalmon, turnep, or potatoe; ſix hours afterwards the animal was killed; the tubes were found in the firſt ſtomach. The fiſh and fleſh were unaltered, whereas the turnep and potatoe were quite diſſolved.

XXI. The ſame experiment being repeated with the ſame ſubſtances boiled, afforded the ſame reſult. The vegetables were digeſted, and the beef and ſalmon unchanged.

XXII. Having found that the ſheep digeſts vegetables very readily, but is incapable of diſſolving animal ſubſtances, I had next recourſe to the ox. Four tubes, one containing raw beef, another fiſh, a third chopped hay, and the fourth leaves of pot-herbs, were given to an animal of this ſpecies, and it was killed ten hours afterwards. The tubes lay in the firſt ſtomach; the fiſh and fleſh were not altered; but I could find no remains of the hay or herbs.

Many experiments of the ſame kind were made upon this animal, and they led me to the ſame concluſion, viz. that the gaſtric fluid of the ox kind, eaſily and ſpeedily diſſolves vegetables, but is incapable of producing this effect upon animal ſubſtances.

In all theſe experiments, I attribute the ſolution of the food to a powerful menſtruum ſecreted by the coats of the ſtomach. It may be objected, that my experiments do not clearly ſhew whether the food is concocted by the gaſtric fluid, or by fermentation, for both cauſes may act equally upon aliment incloſed in the ſpheres. But beſides the arguments already adduced to ſhew (*a*), that fermentation does not produce this effect, many circumſtances attending theſe experiments, clearly ſhew the efficacy of the gaſtric liquor. For in the experiments in which the food was not quite diſſolved, the ſolution always began at the ſurface, and proceeded towards the center, and what remained, ſhewed no tokens of fermentation.

In the XIIIth experiment ivory was diſſolved, while parſnep, a vegetable of ſoft tex-

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(*a*) In the part that has been omitted.

texture, and liable to fermentation, was not at all altered.

To remove every doubt, the following experiment was ſeveral times repeated, and always afforded the ſame reſult.

XXIII. Having kept a dog faſting eighteen hours, that his ſtomach might be free from the remains of food, I killed it, and collected about half an ounce of pure gaſtric fluid, which was put into a phial with twelve grains of roaſt beef. The ſame quantity of the ſame beef was put into another phial, containing water, in order to ſerve for a term of compariſon. Both phials were placed in a furnace, of which the temperature was equal to 102—104° of Fahrenheit's thermometer. In eight hours the beef in the gaſtric fluid was quite diſſolved, whereas that in the water had undergone no perceptible alteration.. In twenty-four hours both phials were taken out of the furnace and carefully examined. The food diſſolved in the gaſtric fluid emitted a rancid and pungent, but by no means a putrid odour; it reſembled very much the ſmell of burnt feathers. The meat in the other phial was quite putrid, and intolerably fetid; but its bulk was not diminiſhed.

I carefully obſerved the phial containing the gaſtric fluid during the ſolution, but could perceive no air-bubbles ariſing, or any other token of fermentation. I repeated this experiment with maſticated meat, when the ſolution was much more ſpeedily completed.

I afterwards made trial of mutton, veal, lamb, and other animal, together with a great variety of vegetable ſubſtances; all were eaſily diſſolved; but the time requiſite for the completion of this proceſs was different, and anſwered exactly to the reſults of the preceding experiments.

As in this experiment there was no ſign of fermentation or putrefaction, I ſuſpected that the gaſtric fluid, as well as the ſaliva (*a*), retards both the one and the other. In order to determine this, I made the following experiment.

XXIV. I took two alimentary mixtures, each conſiſting of mutton and bread in equal quantities. Upon one, half an ounce of the recent gaſtric juice of a dog was poured, and upon the other the ſame quantity of pure water. Both mixtures were beaten to a pulp, and incloſed in phials accurately ſtopped; they were then ſet in a furnace, heated to the 102nd deg. of Fahrenheit's thermometer. Fermentation took place in a few hours in the phial that contained the water, the ſolid contents roſe to the ſurface, and air was extricated with a conſiderable inteſtine motion. The mixture immerſed in the gaſtric fluid, remained fourteen hours with ſcarce any tokens of fermentation; but a ſhort time afterwards, this proceſs evidently took place. The bread and fleſh aroſe to the ſurface of the mixture, a ſediment began to be depoſited, and air-bubbles were continually extricated. But theſe phænomena continued much longer than in the other phial; the commotion

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(*a*) Where did the author learn that the ſaliva checks fermentation? It appears to forward it both from the experiments of Pringle, Macbride, Spallanzani and others. T.

was leſs violent, and the air was not ſo rapidly extricated. When the fermentation had entirely ceaſed, the taſte of the mixture in this phial was indeed acid, but not ſo ſtrong as in the other, and it was converted into a fluid by the ſolvent power of the gaſtric liquor.

XXV. I divided a piece of putrid mutton into two parts, each of which was put into a ſeparate phial, and to one, half an ounce of the recent gaſtric fluid of a dog was added, and to the other, which was deſigned as a term of compariſon, as much water. They were ſet in a cool place, and two days afterwards I examined them, when the latter emitted an intolerably putrid ſmell, and the other, though it had yet a bad odour, did not ſmell ſo diſagreeably as the preceding, nor even ſo diſagreeably as at firſt. Upon ſhaking the phial, the meat fell to pieces, but it was not quite diſſolved. This, perhaps, happened, becauſe it was not expoſed to a ſufficient heat.

Theſe experiments throw great light on digeſtion. They ſhew, that it is not the effect of heat, trituration, putrefaction, or fermentation alone, but of a powerful ſolvent, ſecreted by the coats of the ſtomach, which converts the aliment into a fluid, reſembling the blood. If it ſhould be aſked, what defends the organ itſelf, I would anſwer, that it is the vital principle, as Mr. Hunter's (*a*) obſervations ſhew; after death it is diſſolved

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(*a*) Philoſoph. Tranſ. for 1772. The ingenious obſerver ſeems, however, to attribute too much to this diſſolved as readily as any other inanimate ſubſtance. It is probable, that every ſpecies of animal has its peculiar gaſtric liquor, capable of diſſolving certain ſubſtances only. Some living ſolely upon vegetables, others upon animals, and theſe cannot be obliged to feed upon plants, by a faſt of whatever continuance. All, by an infallibe inſtinct, chooſe what is beſt adapted to their gaſtric fluid. The food, when diſſolved, is expelled from the ſtomach, and being mixed with

this principle. He ſuppoſes, that whatever poſſeſſes it, is capable of reſiſting the action of the gaſtric liquor; his arguments by no means prove this. Worms, indeed, live in the human ſtomach, but it does not follow, that other animals alſo can, for nature may have given them a particular ſtructure of body. The following conſiderations will render the general propoſition very doubtful. Fiſhes ſwallow and digeſt living crabs, lobſters, &c. The leech is concocted by the human ſtomach, though it has no pores, and can ſuſtain a temperature equal to that of man. Cornelius found a ſnake half digeſted in a bird's ſtomach, but ſtill alive. Plot ſaw one eye conſumed, while the fiſh was alive. It ſeems therefore probable, that the gaſtric liquor acts alſo upon living things. Perhaps, likewiſe, it is ſometimes ſo changed, as to act on the ſtomach itſelf. The following caſes communicated by Dr. Monro render this probable. A lady, that uſed to complain of pain in the ſtomach, died ſuddenly. Upon opening the body, a hole was found in the left ſide, and the coats were relaxed as if they were half putrified. There were no appearances of gangrene. A boy died after having long ſtruggled with ſimilar pains. The ſtomach exhibited the very ſame appearance, if we except the hole. From the preceding ſymptoms, one may venture to ſuppoſe, that ſome alteration was produced before death. But this is only conjecture, and future experiments muſt determine the queſtion.

the bile and pancreatic juice in the duodenum, is changed into a mild blood and inodorous liquid, which is denominated chyle. The chyle is abſorbed by numberleſs veſſels, and is carried by the thoracic duct into the ſubclavian vein, in order to repair the conſtant waſte of the body.

EXPERIMENTS ON DIGESTION,

By Mr. GOSSE,

As they are related by Mr. Senebier, in his Conſiderations on the Mode in which Spallanzani conducted his Reſearches on the ſame Function.

THE experiments of Spallanzani on Digeſtion ſo clearly illuſtrate the ſubject, as to leave nothing further to be deſired reſpecting the concoction of ſolid food! And though his experiments on himſelf put the finiſhing hand to his reſearches, they are not however ſufficient to ſatisfy every queſtion that may be aſked.—For what happens to the food while it continues in the ſtomach? When does digeſtion begin? When does it end? What kind of food is moſt eaſy of digeſtion? To ſuch queſtions, the event of experiments made with ſilver tubes ſwallowed full of meat, and afterwards voided with the excrements, can afford no ſatisfactory reply. To aſcertain theſe particulars, it was neceſſary to be able to vomit at will, without injuring the ſtomach, an advantage really poſſeſſed by Mr. Goſſe. The experiments of this gentleman were made with the utmoſt care; and being ignorant of the reſearches of Spallanzani, and having adopted no ſyſtem, he obſerved the phænomena only; but theſe he obſerved with the eye of an experienced naturaliſt, an able chemiſt, and an ingenuous philoſopher, who regarded truth above all things. His inquiries were directed to the benefit of his own health; had he enjoyed greater leiſure, he would have directed them to the advancement of ſcience. Mr. Goſſe, at my requeſt, ſent me an account of his experiments, with permiſſion to inſert them in this preface; and to ſubjoin ſuch obſervations as I might think proper, he himſelf not having annexed any.

OBSERVATIONS ON SWALLOWING ATMOSPHERIC AIR.

Mr. Goſſe, when a boy, acquired the art of ſwallowing air; he conceived the idea, in conſequence of feeling ſome acidity in his ſtomach; the air excited vomiting, which relieved him. He had ever afterwards recourſe, in his indigeſtions, to the ſame remedy, and it conſtantly operated without nauſea or fatigue to the ſtomach: by drinking ſome water, and then throwing it up, he contrived to waſh this organ, as well as if he had held it in his hand. Finding himſelf in poſſeſſion of ſuch an advantage, he thought of making obſervations on digeſtion, which he began to do in 1760.

In order to introduce air into his ſtomach he held his breath, ſhut his mouth, preſſed the air againſt his palate, and thus obliged it to deſcend like any other ſubſtance, by the exertion of the muſcles of deglutition. Its paſſage along the œſophagus was rendered ſenſible, both by the ſpace it occupied, and the noiſe it made.

By this method, in appearance ſo ſimple, though not very eaſy to practiſe, he excited vomiting at pleaſure: he ſuppoſes the effect to be produced by the dilatation of the air, which is occaſioned by the warmth of the ſtomach, for the colder it was, the leſs was neceſſary. Every mouthful of air was nearly equal to a cubic inch.

Two mouthfuls, at a temperature of 4° or 5° above the freezing point, ſwallowed when his ſtomach was empty, occaſioned a painful tenſion, which could only be removed by evacuating the air and taking food. When he wiſhes to vomit ſeveral times in ſucceſſion, he is obliged to ſwallow freſh air every time, till nothing but this fluid unmixed comes from the ſtomach.

We have here a new emetic, ſo gentle in its operation, that it would be of great uſe to the ſick if they could take it, or if an eaſy way of adminiſtering it could be contrived.

GENERAL EXPERIMENTS ON DIGESTION.

I. Mr. Goſſe, being in perfect health, took at dinner ſoup made of lean ſalt beef, with common bread of Paris, and chopped herbs, among which were chevril, borage, and onions; he then eat lean beef with ſalt, and afterwards ſpinach boiled in broth, with the ſame bread baked the day before, and probably fermented with yeaſt; he drank red wine of Orleans. Half an hour after dinner, he ſwallowed ſome air and vomited: he found, that the food, though well maſticated, had undergone ſcarce any change; the ſeveral things he had taken retained their taſte, and their weight was nearly the ſame as at firſt, a ſmall quantity of gaſtric liquor being only mixed with them.

II. Having made the ſame dinner another day, he ſwallowed an equal quantity of air an hour afterwards, and after vomiting, found the food reduced to a pulp, with which there was mixed much gaſtric fluid. The taſte of the aliments was not much changed, except that of the wine, which was now much milder; the gaſtric juice had produced an addition of weight; but though the food had lain an hour in the ſtomach, it did not ſeem to have undergone any degree of fermentation.

III. He repeated the experiment two hours after a dinner of the ſame kind. The aliments were, as before, reduced to a pultaceous maſs, but there was no change of taſte, nor any indication of fermentation; he could, however, recover only half of what he had eaten.

CONSEQUENCES OF THESE EXPERIMENTS.

These experiments agree with thoſe of Spallanzani. The food, we ſee, is not diſſolved till it is mixed with gaſtric liquor:

the juices of the ſtomach act with great celerity, for in about an hour and an half they change the food into a pultaceous matter, and render it fluid, without however altering its nature: when digeſtion is properly carried on, there is no appearance of acidity or alkaleſcence: the food does not all ferment, and the proceſs of digeſtion is not compleated, till between two and three hours have elapſed.

The experiments of Mr. Goſſe confirm a conjecture I had formed, upon the cauſe which obliges the gaſtric fluid to iſſue from the glands by which it is ſecreted. I entertained a ſuſpicion, that the weight of the food forces out this liquor, by ſtretching the internal coat of the ſtomach, juſt as the ſame effect is produced by ſtretching it with the hand after the death of the animal. In reality, the weight of the food is ſcarce changed during the firſt half hour; the tenſion muſt be continued longer, in order that there may be an afflux of gaſtric liquor. Further, when Mr. Goſſe drank milk, it required above half an hour to curdle: whence we may conclude, that at firſt there is no gaſtric fluid in the ſtomach, and that the milk preſſes it out by its weight when it has continued in the ſtomach ſome time.

EXPERIMENTS TO DETERMINE THE DEGREE OF DIGESTIBILITY OF DIFFERENT KINDS OF FOOD.

Mr. Goſſe having obſerved what happens in his ſtomach in the ordinary progreſs of digeſtion, wiſhed to know, by means of vomiting,

ing, the reſpective digeſtibility of various kinds of food, that he might chooſe ſuch as would beſt agree with him. The following is the reſult of his experiments, as divided by him into three claſſes.

The firſt contains ſuch matters as appeared to him not to be digeſtible. The ſecond, ſuch as were in part digeſted. The third, thoſe of eaſy digeſtion.

I. SUBSTANCES NOT DIGESTIBLE, OR SUCH AS WERE NOT DIGESTED IN THE USUAL TIME.

ANIMAL SUBSTANCES.

I. The tendinous aponeurotic parts of *beef*, *veal*, *pork*, *poultry*, *ſcate*.

II. The *bones*.

III. The *oily* or *fatty* ſubſtances of theſe animals.

IV. The *white of an egg* indurated by heat.

VEGETABLE SUBSTANCES.

V. Oily or emulſive ſeeds, ſuch as walnuts, almonds, nuts, pine-kernels, piſtacchio nuts, grape-ſtones, the ſeeds of apples, pears, oranges, lemons, olives and cocoa.

VI. Unctuous oils expreſſed from walnuts, almonds, nuts, and olives.

VII. Dried grapes, well maſticated, remained in his ſtomach for two days.

VIII. The *ſkin* of freſh grapes.

IX. The ſkin or rind of farinaceous ſubſtances, as peaſe, beans, lentil, wheat, barley.

X.

X. The pods of peaſe and kidney beans.

XI. The ſkin of ſtone-fruits, as cherries, apricots, plums, peaches, damaſcenes.

XII. The huſk of fruits with grains or ſeeds, as of apples, pears, oranges, lemons. Conſerves of oranges and citrons, notwithſtanding the preparation they undergo, are very difficult of digeſtion.

XIII. The *capſules* of fruits with grains, as of apples and pears.

XIV. Ligneous ſtones, as thoſe of cherries and plums.

It ſhould be obſerved, that theſe ſeeds do not, any more than the emulſive, loſe their vegetative power by lying in the ſtomach: the germination of ſome is forwarded by continuing there. How many plants fail to grow, becauſe their ſeeds are diſperſed without manure or ſuſtenance? Bitterſweet, *(ſolanum dulcamara)* miſletoe, hemp, and other plants, which ſometimes grow upon trees, are produced by means of the excrement of birds.

II. SUBSTANCES LESS INDIGESTIBLE, PART OF WHICH MR. GOSSE DIGESTED.

ANIMAL SUBSTANCES.

I. *Pork* dreſſed in all the different methods.

II. Black-puddings.

III. Yolk of eggs roaſted.

IV. Fritters of eggs. The eggs almoſt conſtantly acquired an alkaline character, and a taſte of liver of ſulphur, produced by the alkali in the white, and the ſulphur diſcovered by Mr. Deyeux in the yolk.

V. Fried eggs and bacon were very difficult of digeſtion. The fat of the bacon prevented the alkaleſcence of the eggs, but acidity often enſued.

Vegetable Substances.

VI. Sallad of raw herbs, as lettuce, dandelion, creſſes, ſuccory. The bitterneſs of ſome of theſe herbs ſeemed to facilitate digeſtion. The oil and vinegar uſed for ſeaſoning ſallad tend to retard digeſtion, but it is promoted by the ſalt and pepper. Mr. Goſſe could not long continue the uſe of raw vegetables, on account of the acidity they produced.

VII. White cabbage ſeemed leſs eaſy of digeſtion than the red, and the nerves of the leaves, leſs than the parenchymatous ſubſtance.

VIII. Beet, cardoons.

IX. Boiled and raw onions and leeks.

X. Roots of ſcurvy-graſs, red and yellow carrots, and ſuccory, are more difficult to digeſt in ſallad, than in any other way.

XI. The pulp of fruit with ſeeds, when it is not fluid.

XII. Warm bread, on account of the ſtrong acidity it produces.

XIII. Figs, freſh and dry.

XIV. Sweet paſtry occaſioned an inſupportable acidity.

XV. All theſe ſubſtances became more difficult of digeſtion, when they were fried in butter or oil.

Mr. Goſſe further remarks, that theſe ſeveral kinds of food, if they ſhould not be quite diſſolved in the ſtomach, are digeſted in their paſſage through the inteſtines, whether by a continuation of the action of the gaſtric liquor, or on account of the admixture of the bile, pancreatic juice, &c.

III. SUBSTANCES EASY OF DIGESTION, SUCH AS ARE REDUCED TO A PULP IN AN HOUR, OR AN HOUR AND HALF.

ANIMAL SUBSTANCES.

I. Veal, lamb, and in general the fleſh of young animals, is ſooner digeſted than of the old. All kind of *poultry*, and eſpecially ſuch as is young.

II. *Freſh eggs* not hardened by boiling.

III. Cow's milk.

IV. Perch boiled, with a little ſalt, with parſley. When it is well fried, it does not digeſt ſo well, and the ſame thing happens when it is ſeaſoned with oil, wine and white ſauce.

VEGETABLE SUBSTANCES.

V. Herbs, as ſpinach mixed with ſorrel, are leſs eaſily digeſtible. Celery, but the nerves do not yield ſo readily. The tops of aſparagus, hops, and the ornithogallus of the Pyrenees, known by the name of mountain hops.

VI. The bottom, or the placenta of artichokes.

VII. The boiled pulp of fruits with ſeeds and ſtones, ſeaſoned with ſugar, becomes more eaſy of digeſtion.

VIII. The pulp or meal of farinaceous ſeeds, as wheat, barley, rice, maize, peaſe, beans, cheſnuts, &c: roaſted cheſnuts are more difficult of digeſtion.

IX. Different ſorts of wheaten bread, without butter, the ſecond day after baking. The cruſt ſeemed to him more digeſtible than the crumb. The ſalted bread of Geneva, is ſooner digeſted than the Pariſian bread made without ſalt. Bread of rye and buck-wheat does not digeſt ſo well; the ſame obſervation may be applied to brown bread, in proportion as it contains more bran.

X. Rapes, turneps, potatoes, parſneps of good quality, and not too old.

XI. Gum Arabic; but the acid of this ſubſtance is ſoon felt. The Arabians, perhaps, among whom it is an article of food, obviate this effect by ſome preſervative.

SUBSTANCES WHICH FACILITATED MR, GOSSE'S DIGESTION.

I. Sea or common ſalt.

II. Spices, as pepper, cannella, nutmeg, cloves.

III. Muſtard, ſcurvy-graſs, horſe-radiſh, radiſh.

IV. Capers.

V. Wine, ſpirituous liquors in ſmall quantities.

VI. Cheeſe, and more eſpecially old cheeſe.

VII. Sugar.

VIII. Various bitters.

SUB-

SUBSTANCES WHICH RETARDED HIS DIGESTION.

I. Water, particularly when hot, taken in large quantities. It occaſions the food to paſs into the inteſtines, without being properly diſſolved.

II. All acids.

III. All aſtringents; twenty-four grains of Peruvian bark, taken half an hour after dinner, ſtopped digeſtion.

IV. All unctuous ſubſtances.

V. A ſtrong decoction of bitter-ſweet prevented, upon one occaſion, the digeſtion of the moſt digeſtible ſubſtances, and they turned ſour.

VI. A grain of kermes, taken after dinner, produced the ſame effect.

VII. As alſo did a grain of corroſive ſublimate.

Laſtly, He obſerved, that employment, after a meal, ſuſpended or retarded digeſtion; as likewiſe did leaning with the breaſt againſt a table. He remarked that repoſe of mind, a vertical poſition of the body, gentle exerciſe after taking food, were favourable to digeſtion.

The benefit which medicine and the health of mankind may derive from ſuch experiments is obvious. It were to be wiſhed, that they ſhould be repeated and varied, by making trial, for inſtance, of two or three different kinds of fruit at once, &c.

The reſults of Mr. Goſſe's experiments are different from thoſe of Mr. Reuſs, as related in a Diſſertation printed at Edinburgh in 1768. He examined the ſtate of his food three hours after having taken it; but he took an emetic in order to bring it up; now it is obvious, that an emetic diſorders the ſtomach, and that in three hours, food of every kind muſt undergo too great changes, to ſhew the reſpective digeſtibility of various ſubſtances.

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END of the FIRST VOLUME.

TRANSLATION

OF

ITALIAN, LATIN, &c. PASSAGES.

Page 117. *Come le rane*, &c. As frogs ſcour along the water, at the approach of the water-ſnake, without ſtopping, till they have gained the dry ground.

Page 202. *Receptum eſt*, &c. It is commonly ſuppoſed that animals can digeſt bone: Boerhaave formerly followed the opinion of Van Helmont. To attain greater certainty, he obſerved with care what happens to food in the ſtomach of animals of powerful digeſtion: he found that bone is not digeſted.

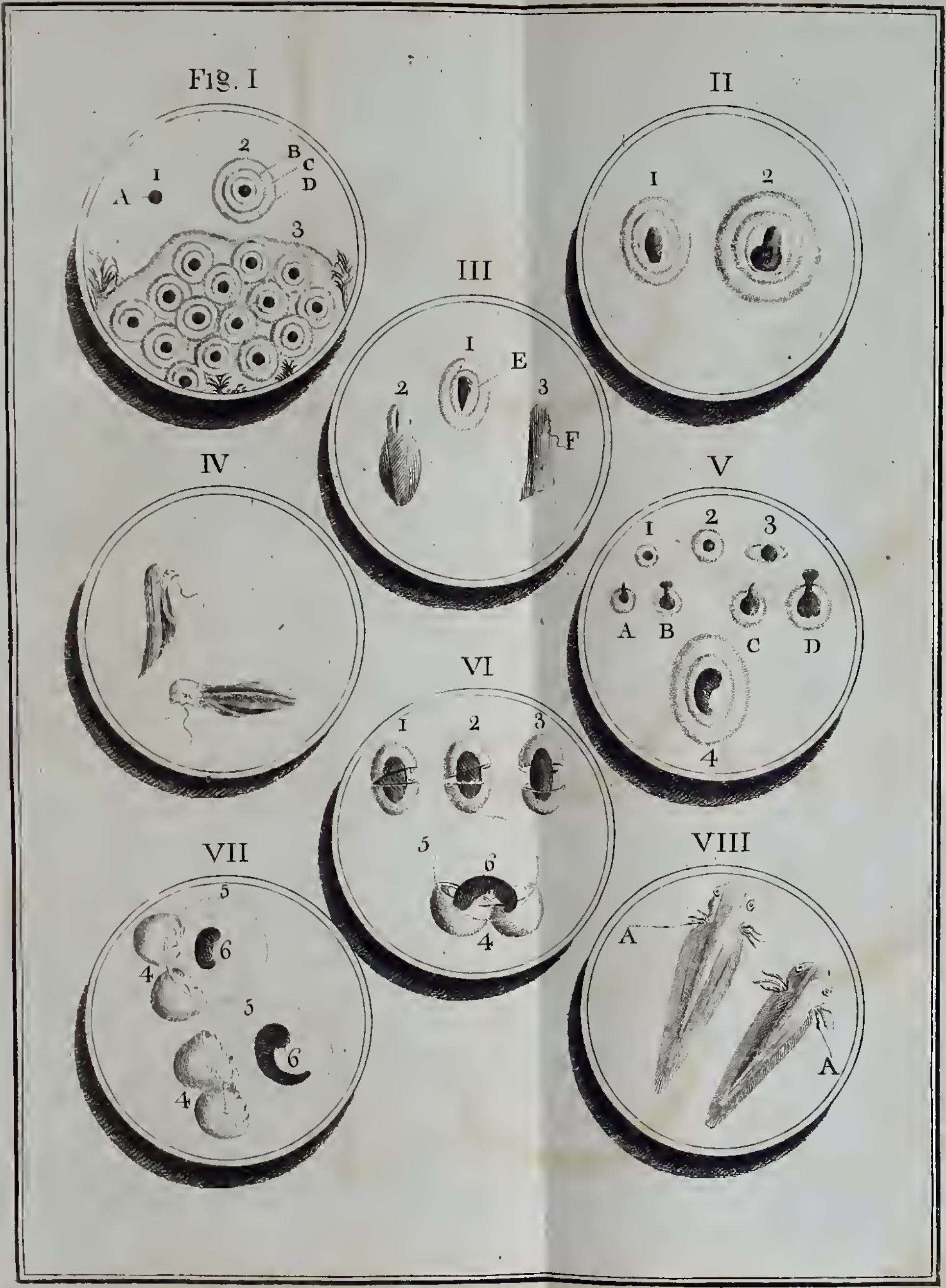
He gave an hungry dog ſome inteſtine, which were eagerly devoured, but inſtead of being concocted, they were partially voided, and hanging out at the anus, tormented the poor animal. He afterwards gave another dog ſome bones anointed with butter; but nothing was diſſolved, except what water will diſſolve. Of fleſh, the dry and expreſſed fibres were voided, and ligament, in paſſing through the alimentary canal, underwent no change.

Page 208. In the excrement of the dog, we find fragments of bone little altered. It is merely reduced to pieces; the ſucculence is expreſſed, and the fragments are formed into one maſs.

E R R A T A.

P. 8. l. penult. and elſewhere, for *turkies* read *turkeys*—P. 31. l. 23. for *one only* read *only one*—P. 48. l. 24. for *abounding* read *abound*—P. 62. l. 18. for *neck* read *beak*—P. 66. l. 6. for *canem* read *carnem*—P. 70. l. 16. between *them* and *being* inſert *from*—P. 78. note, for 42 read 41—P. 83. l. 21. between *beat* and *when* inſert *as*—P. 90. l. 6. for *are* read *is*—P. 97. l. 13. for *inſinuating* read *inſinuate*—P. 109. l. 11. for *rooks* read *ravens*—P. 110. l. 21. for *came* read *come*—P. 118. l. 15. between *wiſhed* and *very* inſert *it*—Ditto l. 16. for *progreſs* read *proceſs*—P. 124. l. 4. for *in* read *into*—P. 127. l. 32. for *ejected* read *rejected*—P. 128. l. 48. for *barbels* read *barbel*—P. 135. l. 21. for *or* read *nor*—P. 140. l. 15. for *axillæ* read *axilla*—P. 143. l. penult. for *they* read *it*—P. 152. l. 5. for *where* read *when*—Ditto l. 24. for *crow* read *owl*—P. 155. l. 33. for *owl* read *ſcreech-owl*—P. 163. l. 15. after *for* inſert *as*—P. 166. l. 26. for *in* read *to*—P. 171. l. 32. for *beheld* read *behold*—P. 172. l. 10. dele *one*—P. 173 l. 19. after *only* inſert *would*—P. 184. l. 24. for *were* read *was*—P. 201. l. 16. before *appeared* inſert *as*—P. 207. l. 4. after *examine* inſert *them*—P. 215. l. 11. for *are* read *is*—P. 219. l. 21. for *analogus* read *analogous*—P. 231. l. 33. dele *if*—P. 274. l. 7. for *Gardana* read *Gardane*—P. 290. l. 14. after *in* inſert *not*—P. 292. l. 30. for *choſe* read *chooſe*—P. 293. l. 25. dele *of*—P. 206. l. 21. for *miaſma* read *magma*—P. 311. l. 27. the words *in the part that has been omitted* ſhould have been at the bottom of the page—P. 316. l. 2. dele *blood*.

PL. I.



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Pine sc